

*Accounting  
and Business  
Research*

# Accounting and Business Research

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# Accounting and Business Research

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## Editorial

It is the dream of every editor that circulation will increase, the standard of articles published get even better and publishing costs not rise any further. This last is, alas, only a dream these days and it has now become necessary for *Accounting and Business Research* to follow the example of other academic journals in asking a submission fee of its contributors. A submission fee will *not* however, be charged to subscribers. Anyone who wants to publish in *Accounting and Business Research* should also think it worth subscribing to, and we look forward not to a large revenue from submission fees but to a larger circulation. The submission fee will be £10 (\$20) and will be payable as from 2 April 1979.

Observant readers will have noted that the Autumn 1978 issue was 64 pages long, thus departing from our norm of 80 pages per issue. The current issue contains 96 pages. It is expected that the special Accounting History issue in 1980 will also contain 96 pages.

The time has come round for subscribers to vote for the best article of 1977/78 and a Walter Taplin Prize leaflet accompanies this issue. We value our subscribers' opinions and hope that many of them will take this opportunity to vote for the article of their choice.

Finally, we would like to draw our readers' attention to the recent publication of *Readings in Accounting and Business Research 1970-1977*. Further details and an order form can be found on the inside back cover of this issue.

# Accounting Determinants of Systematic Risk in Canadian Common Stocks: a Multivariate Approach

Ahmed Belkaoui

The literature of finance theory and particularly work with the capital asset pricing model has identified a systematic or market risk and a specific or diversifiable risk. One interesting research question has been the degree of association between the common stock systematic risk and accounting determined risk measures. Different conceptual studies argue that differences in systematic risk between firms may be due to differences in the corporate financial decision [1, 2, 5, 13, 14]. In other words, it is thought that corporate risk variables derived from accounting data resulting from corporate decisions may convey information about the magnitude of the systematic risk of a common stock. Empirical evidence, mainly for the US market, is still inconclusive. Similar evidence for the Canadian market is either rare or suffers from serious methodological shortcomings [3, 4, 9, 19]. Consequently the purpose of this paper is to provide some Canadian empirical evidence on the relationships between accounting determined and market determined measures of risk. With a knowledge of the accounting determinants of the systematic risk or beta, business managers may be able better to assess the relevance of their particular corporate decisions. Similarly, the investing public may be able better to predict any future variations in the firm's systematic risk on the basis of published accounting numbers. Finally, a positive association between beta and accounting risk measures will provide evidence for the semi-strong efficient market hypothesis for the Canadian market in the sense that market prices reflect all publicly available information [10]. In other words, any association will lend support to the hypothesis that accounting data reflect the events that first determine a security's riskiness and second are also reflected in its market price. The issue is of major importance to all users

basing their valuation models on accounting numbers.

## Related research

The study of the association of individual firms' risk with their underlying accounting characteristics started with the seminal study of Beaver, Kettler, and Scholes [2] which discovered significant relationships between  $\beta$  and dividend payout, financial leverage, and earnings yield instability measures. Gonedes [13], however, did not find a strong relationship between the regression parameters of his accounting model and the risk coefficient of the market model. He explained the difference between both studies on the basis of the differences of the accounting ratios used. He scaled income numbers by another accounting number, viz., total assets, while Beaver, Kettler and Scholes scaled income numbers with market prices. Using his words:

I suppose that 'The significant associations' reported by Beaver, Kettler, and Scholes are direct results of the fact that their so-called 'accounting-based' estimates of earnings covariability are actually functions of market prices because they used market prices to scale income numbers.... [13, p. 436]

However, in another study, Derstine and Huefner [7] using both market and accounting scaled data, found significant correlation in both cases. The objectives of their study were however different. First, they found that alternative accounting methods (specifically LIFO and FIFO) did not affect intercompany comparisons of accounting ratios. Second, in ranking companies on the basis of accounting data and market determined risk, the LIFO—FIFO choice did not create a significant difference.

The results of Beaver, Kettler and Scholes were

verified using a similar set of explanatory variables on cross-section monthly regressions by Breen and Lerner [5]. In their study, only the regression coefficients of the stability of earnings growth, size, payout, earnings growth rate, and number of shares traded had the expected sign and were statistically significant. Their results on the instability of the sign, magnitude and statistical significance of the leverage factor contradicted the results of Hamada's study [14], which concluded that leverage accounts for 21% to 24% of the systematic risk. Along the same lines other different studies focused on the link between beta and financial data. Rosenberg and McKibben [20] found that 13 out of 32 variables examined were significant. Logue and Merville [15] found financial leverage, return on assets, and asset size variables to be significant. Most recently, Beaver and Manegold [1], Bildersee [6], Melicher [18], Melicher and Rush [17], and Thompson [22] reported results supporting the association between accounting determined and market determined risk measures.

The present paper differs from previous studies in several respects:

- (1) Contrary to previous studies, all of which employ US data, this paper is restricted to Canadian firms.
- (2) In most previous studies, with the exception of Melicher [18] and Melicher and Rush [17], the multicollinearity between financial ratios was not corrected.<sup>1</sup> This paper will use factor analysis to identify the main financial dimensions.
- (3) To correct for the short term instability of individuals, a single four year market period, January 1st, 1971 to December 31st, 1974, was chosen as the period of analysis. [4]
- (4) Similarly, most of the previous research, except for Breen and Lerner [5], focuses on the University of Chicago Center for Research in Security Price File ending December 1968, which might be biased by the bull market of the 1960s and hence does not portray the economic environment of the 1970s.

## Risk measures employed

### Systematic Risk

The capital asset pricing model as presented by Sharpe [21], Lintner [16], and Fama [11], asserts that in equilibrium, and under certain conditions, the risk premium for an individual secur-

ity,  $E(\tilde{R}_i) - E(\tilde{R}_F)$  is related to the risk premium of the market,  $E(\tilde{R}_m) - E(\tilde{R}_F)$  by the expression:

$$E(\tilde{R}_i) - E(\tilde{R}_F) = [E(\tilde{R}_m) - E(\tilde{R}_F)]\beta_i$$

where

$E(\tilde{R}_F)$  = risk-free rate

$E(\tilde{R}_m)$  = expected return on a market factor

$\beta_i = \text{cov}(\tilde{R}_i, \tilde{R}_m) / \text{var}(\tilde{R}_m)$

$\beta_i$  is a measure of the systematic or non-diversifiable risk. Its estimation is operationally possible using the one factor market model, which asserts a linear relationship between the rate of return on security  $i$ ,  $R_{it}$ , and the market rate of return,  $R_{mt}$ , for a period  $t$ . It is expressed in this study as follows:

$$\begin{aligned} r_{it} &= \alpha_i + \beta r_{mt} + e_{it} \\ E\{e_{it}\} &= 0; \\ E\{e_{it}^2\} &= N^0; \\ E\{e_{it} \cdot e_{ik}\} &= 0, \forall k \neq t; \\ E\{e_{st} \cdot e_{it}\} &= 0, \forall s \neq i; \\ E\{\ln\{r_{mt}^2\} \cdot e_{it}\} &= 0 \end{aligned}$$

where

- $r_{it}$  = continuously compounded rate of return of security  $i$  at period  $t$   
 $= \log_e(1 + R_{it})$   
 $= \log_e[(P_t + D_t)/P_{t-1}]$
- $R_{it}$  = non-compounded single period return of security  $i$  in period  $t$ .
- $r_m$  = market factor in period  $t$   $\log_e(TSEI_t/TSEI_{t-1})$
- TSEI = Toronto Stock Exchange Index
- $e_{it}$  = logarithm of the residual term
- $D_{it}$  = cash dividend per share
- $\alpha_i, \beta_i$  = parameters of the least squares regression

$r_{it}$  is used instead of  $R_{it}$  because it is admitted that, first,  $r_{it}$  has fewer outliers in its relative frequency distribution and therefore will yield more efficient risk statistics than  $R_{it}$ , and second,  $r_{it}$  is distributed more symmetrically than the positively skewed  $R_{it}$  variable [12]. Besides, the results of the Sharpe's Model are not changed by restating them in terms of  $r_{it}$  instead of  $R_{it}$ .

### Accounting-based measures of risk

Although most accounting ratios are not defined in terms of covariance of returns, they may be perceived by financial users as a reflection of the uncertainty of the earnings stream of a firm. This rationale for the perception of accounting ratios as surrogates for the total variability of return

<sup>1</sup>Thus, specification bias may exist in most multivariate studies.

of common stocks may lie in the socialisation and learning process. Most finance textbooks and most techniques of financial analysis and share valuation rely on ratio analysis for decision making and resource allocation. An *a priori* belief is created, whether in the classroom or in the market place, in the presumed usefulness of accounting ratios as determinants of systematic risk. In other words, the users have been socialised into thinking of accounting ratios as surrogates for the total variability of return of common stocks.

A more conceptual rationale for the perception of accounting ratios as surrogates for systematic risk may also be implied by the capital asset pricing model. It may be said on the basis of the implications of the capital asset pricing model that the systematic risk of common stock is related directly to financial leverage, inversely to liquidity, profitability and activity. So, accordingly, the ratios chosen as a group measured financial qualities such as profitability, liquidity, leverage and activity and are listed in Table 1. These variables are presumed to reflect the results of the main corporate decisions most likely to be associated with the systematic risk of the firm.

**Table 1**

**List of Ratios**

1. Common Stock  
Total Assets
2. Net Working Capital  
Total Assets
3. Operating Income + Extraordinary Gains and Losses  
Total Assets
4. Cost of Sales  
Total Assets
5. Cash Dividends  
Income for Common
6. Current Assets  
Current Liabilities
7. Long Term Debt  
Common Equity
8. Long Term Debt  
Total Assets
9. Total Liabilities + Preferred Stock  
Common Equity
10. Net Income Flow Through Base  
Net Share Equity
11. Net Income Deferred Credit Base  
Net Share Equity
12. Net Income Flow Through Base  
Sales
13. Net Income Deferred Credit Base  
Sales
14. Net Working Capital  
Sales
15. Cost of Sales  
Inventory

All the 15 variables were based on the arithmetic averages of the 4 year period 1971–1974.

## Data and methodology

Fifty-five Canadian companies were employed in this study (See Appendix A). The selection criteria were: (1) All the stocks were continuously listed on the Toronto Stock Exchange for the period of analysis, January 1st, 1971, to December 31st, 1974. (2) Besides the availability of continuous accounting data, firms were chosen so as to achieve a wide representation of industries.

An important problem ignored in most previous studies is the possible multicollinearity between the accounting data. This produces possible biases. Consequently, the following methodology was used:

- (1) The  $\beta$  of the group of firms was estimated by regressing a time series of historical returns from the Canadian stock on the returns from The Toronto Stock Exchange Industrial Index.
- (2) A factor-analytic procedure enabled the identification of the basic financial dimensions represented by the accounting ratios.
- (3) The  $\beta$  of the group of firms was finally regressed against the reduced accounting ratio set obtained in step 2.

## Empirical results

### *Stationarity of Common Stock Systematic Risk*

Table 2 lists the distribution of beta for 7 periods: 1971, 1972, 1973, 1974, 1971–72, 1973–74, and 1971–74. They were computed from bi-weekly return data. This table summarises the distribution of beta in each of the seven periods in terms of high, low, mean, medium, standard deviations and quartile points. The number of betas less than zero is also presented. Over the period of interest to this study, 1971–1974, the mean was 0.5681, the median was 0.5373, and the first and fourth quartile points respectively were 0.03675 and 1.2893. It is also appropriate to note that during the same period only 7 companies or 11.29% have betas less than zeros.

To examine the stationarity of betas, correlation tests examined the association between beta values in successive market periods, thus performing four correlation studies, 1971 and 1972, 1972 and 1973, 1973 and 1974 and 1971–72 and 1973–74. Table 3 lists the correlation coefficient between the successive betas. The beta correlation

**Table 2**  
**Distribution of Beta Coefficients**

Period	Mean	Standard Deviation	Number of Betas > 0	Number of Betas < 0	Low	0.10	0.25	0.50	0.75	0.90	High
1971	0.3714	0.6410	50	12	-2.8412	-0.1188	0.1099	0.3559	0.7621	1.1548	1.5033
1972	0.7384	0.8252	47	15	-0.54933	-0.2864	0.06113	0.73818	1.3083	1.9197	3.2623
1973	0.6029	0.8956	50	12	-1.8982	-0.24307	0.2023	0.6336	1.1914	1.6556	2.6561
1974	0.6180	0.8479	51	11	-2.374	-0.3689	0.1801	0.6800	1.1229	1.8118	2.804
1971-72	0.5080	0.5751	52	10	-1.6708	-0.1424	0.09811	0.5342	0.8069	1.3577	1.723
1973-74	0.6269	0.7065	53	9	-1.0992	0.1689	0.1815	0.6797	1.0666	1.4644	2.6912
1971-74	0.5681	0.5183	55	7	-1.1993	-0.03675	0.2618	0.53731	0.8858	1.2893	2.0383



**Table 3**  
**Correlation Coefficients for Successive Betas**

<i>Periods</i>	<i>Correlation Coefficient <math>\beta</math></i>
1971 $\times$ 1972	0.3835 (2.828)*
1972 $\times$ 1973	0.5888 (4.7764)*
1973 $\times$ 1974	0.27385 (2.0178)
1971-72 $\times$ 1973-74	0.4786 (5.340)*

( ) t values.

\* Significant at 0.01 level.

coefficients were significant at 0.01 level for three periods out of four.<sup>2</sup>

#### *Factor Analysis Phase*

Although the multivariate approach to the study of the determinants of systematic risk presents intuitive appeal by allowing the consideration of more than one explanatory variable, there is always the risk that the accounting ratios as independent variables may be mutually correlated. This departure from one of the assumptions of the linear model, known as multicollinearity, may impair the results. An efficient way to resolve the problem is to submit the original ratio set to a factor analytic procedure, both to simplify and group patterns in the data. Factor analysis

is a generic name for a class of techniques which permits data reduction and summarisation without appreciable loss of information. It reduces the number of variables in terms of new, uncorrelated factors. The factor patterns explain parsimoniously the observed data and retain the most important information contained in the original data matrix. Since multicollinearity problems were experienced in this study, the original set of ratios was factored into five distinct and orthogonal dimensions, with each dimension being a linear combination of the original fifteen ratios. The computer program employed was the BMD08M [7]. The first five factors shown in Table 4 were retained as the main dimensions by applying a decision rule which required an eigenvalue of at least 1.00 for a factor to be retained.<sup>3</sup> These five factors accounted for 71.7% of total variance. Information about the grouping of the fifteen ratios with each factor is presented in the factor loadings matrix in Table 5. Only the highest factor loadings per ratio is shown.

An examination of the grouping of certain ratios with the factors and their corresponding factor loadings leads to the labelling of the five factors as (1) profitability, (2) leverage, (3) liquidity, (4) activity, and (5) dividend policy. The factor analysis reduced the original set of mutually correlated fifteen ratios into five uncorrelated dimensions. The decision was made to choose from each of these five factors the ratio showing the highest loading on the factor, to form the reduced set

<sup>2</sup>A more exhaustive presentation of empirical evidence on the market model applied to Canadian common stocks is presented in [3, 4].

<sup>3</sup>See Rummel [21] for a complete discussion of factor analysis.

**Table 4**  
**Summary of Factor Analysis for 15 Ratios**

<i>Factor</i>	<i>Eigenvalue</i>	<i>Percent Variance Explained</i>	<i>Cumulative Proportion of Total Variance Explained</i>
1	3.35217	0.223	0.223
2	2.344	0.156	0.379
3	1.976	0.132	0.511
4	1.679	0.112	0.623
5	1.417	0.094	0.717
6	0.899	0.074	0.791
7	0.753	0.050	0.841
8	0.567	0.038	0.879
9	0.542	0.036	0.915
10	0.443	0.030	0.945
11	0.388	0.025	0.970
12	0.227	0.016	0.986
13	0.117	0.007	0.993
14	0.074	0.005	0.998
15	0.0165	0.001	0.999

**Table 5**  
**Factor Patterns among the Ratios of Canadian Firms\***

<i>Accounting Ratios</i>	<i>Factor Loading</i>				
	1	2	3	4	5
1. Common stock/total assets		-0.414			
2. Net working capital/total assets					0.662
3. Operating income—extraordinary gains and losses/total assets	0.692				
4. Cost of sales/total assets				0.881**	
5. Cash dividends/income for common stock					0.783**
6. Current assets/current liabilities			0.762**		
7. Long term debt/common equity		0.941**			
8. Long term debt/total assets		-0.446			
9. Total liabilities + preferred stock/common equity		0.929			
10. Net income flow through/net share equity	0.917				
11. Net income deferred credit base/net share equity	0.935**				
12. Net income flow through base/sales				-0.802	
13. Net income deferred credit base/sales				-0.742	
14. Net working capital/sales			0.649		
15. Cost of sales/inventory					0.806

\*The highest factor loadings per ratio is shown.

\*\*The highest factor loadings per factor.

of accounting ratios for the next phase of the analysis.<sup>4</sup>

#### *Multiple Regression Phase*

The basic explanatory relationship of this study was

$$\beta = a + b_1x_1 + b_2x_2 + b_3x_3 + b_4x_4 + b_5x_5 + M \quad (1)$$

As indicated earlier the independent variables finally chosen were those ratios having the highest factor loadings on each of the five dimensions identified in the factor analytic phase. They are as follows:

- $x_1$  = Cost of sales/total assets
- $x_2$  = Cash dividends/income for common stock
- $x_3$  = Current assets/current liabilities
- $x_4$  = Long term debt/common equity
- $x_5$  = Net income deferred credit base/net share equity

The parameters of the equation (1) were estimated by Ordinary Least Squares regression, the results

of which are reported in Table 6. The results are interesting on two points:

(1) All the variables showed significant relationships with the estimated beta. Systematic risk for Canadian firms as measured by their betas is negatively related to their measures of activity, dividend payout, and profitability, and positively related to their leverage and liquidity. Noteworthy is the choice of a 0.10 confidence level to infer a significant relationship with beta for both the leverage and the activity ratios. It might be due, as suggested in other studies, to a misspecification of the true relationship between beta and both the leverage and activity ratios [18]. A linear relationship was maintained in this study because of the lack of conceptual justification for a non-linear or quadratic relationship.

(2) The empirical results are all in the direction implied theoretically by the CAPM except for the liquidity ratio. A significant positive relationship is found between the current ratio and estimated beta, contradicting established belief of a negative relationship. Intuitively, one might infer that although increased liquid assets holdings reduce the risk of technical insolvency and lower the firm's beta, the current ratio contrary to popular beliefs is not a good measure of liquidity. Current assets, as a

<sup>4</sup>Although the substitution of the ratios for the factors reduced the data requirements, some correlation among the ratios is reintroduced.

**Table 6****Step-wise Regression Results (with t values in parentheses)**

$$y = 0.5338 - 0.0090 x_1 - 0.1016 x_2 + 0.0915 x_3 + 0.00321 x_4 - 1.3806 x_5$$

$$(-1.276)^{***} \quad (-1.610)^{**} \quad (2.894)^* \quad (1.290)^{***} \quad (-2.057)^{**}$$

F value = 8.352

 $R^2 = 0.3410$ 

where y = Beta

 $x_1$  = Cost of sales/total assets $x_2$  = Cash dividends/income for common stocks $x_3$  = Current assets/current liabilities $x_4$  = Long term debt/common equity $x_5$  = Net income deferred credit base/net share equity

\*Significant at the 0.01 level.

\*\*Significant at the 0.05 level.

\*\*\*Significant at the 0.10 level.

numerator to the current ratio, includes not only cash and short term negotiable instruments but also accounts receivables and inventory amounts. Although the cash and short term negotiable assets may be considered similar to holdings of risk-free assets, accounts receivables and inventory amounts are far from being risk-free. Consequently, the current ratio, as a measure of the shift from liquid assets to current operating assets, will have a positive relationship with the systematic risk of a firm. The results in this study seem to verify this point.

## Conclusion

The Canadian evidence based on examination of the data of 55 Canadian firms supports the contention that accounting based measures of risk are impounded in the systematic risk of common stocks. A significant positive relationship was found between both the current ratio and the long term debt to common equity and the systematic risk. A significant negative relationship was also found between the cost of sales to total assets, the cash dividends to income for common stocks and the net income deferred credit base to net share equity and the systematic risk. In conclusion, both conceptually and empirically it may be stated that the systematic risk of Canadian common stocks is related directly to financial leverage and inversely to profitability and activity; and, contrary to conceptual findings, liquidity as expressed by the current ratio was found to be directly related to the systematic risk.

Although restricted to a small sample of Canadian firms and to only a 4 year period of analysis, the results may be of help to those financial managers speculating about the impact of their

financial policies on the systematic risk of their firms' common stock.

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## Appendix

### *List of Companies*

1. Acklands
2. Woodward Stores
3. Crush International
4. Dominion Stores
5. Molson
6. Gulf Oil
7. Metropolitan Stores
8. Moore Corporation
9. BC Sugar
10. Canadian Marconi
11. Labatt
12. Transair
13. Becker Milk
14. Atlo Industries
15. Silverwood Industries
16. Bell Canada
17. Loblaw's
18. Hayes Dana
19. Union Gas
20. Revelstoke Cos.
21. Bow Valley
22. Greyhound
23. Kelly Douglas
24. Selkirk Holdings
25. Metropolitan Gas
26. Consumer Gas
27. Brascan
28. Thomson Newspapers
29. R. L. Crain Ltd.
30. Southam Press
31. Simpson Sears Ltd.
32. ITL Industries
33. Hudson Bay
34. Abitibi Paper Co.
35. Russel Hugh
36. Versatile Manufacturing
37. BC Forest Products
38. Canadian Tire
39. Interprovincial Steel and Pipe
40. Steel Company of Canada
41. Massey Ferguson
42. Loeb M.
43. Algoma Steel Corporation
44. CAE Industries Ltd.
45. Alcan
46. Canadian Corporate Management Co. Ltd.
47. Zellers
48. Steinberg Ltd.
49. Slater Steel Industries
50. Rothams
51. Leigh Instruments
52. Great Lakes Paper and Pulp
53. EMCO
54. Asbestos Corporation Ltd.
55. Oshawa Group Ltd.

# Linear Programming Dual Prices in Management Accounting and their Interpretation

Susan Dev

Mathematical programming has found its way increasingly into the accounting literature in recent years.<sup>1</sup> In particular, it has been suggested that dual prices can be used explicitly in the management accounting framework and that they have a part to play in the decision-making processes of the firm in that context.

The application discussed in this paper is that of product selection, e.g., where a multi-product firm whose objective is to maximise profit is compiling its budget for a single period, say the coming year. Very simple situations that do not require integer solutions will be considered:<sup>2</sup> the aim is to focus attention on the interpretation of dual prices for factor inputs (such as materials, direct labour and machine time) and, later, on dual prices for demand.

The paper considers two aspects of the inter-

pretation of dual prices in this type of situation to which little attention has been given in the literature. First, it tends to be assumed almost without discussion that effective input constraints will be present in practice. The consequences of this assumption will be examined in the first part of the paper. Dual prices for demand get scant attention in the literature and their interpretation will be considered in the second part, using Carsberg's example of Cygnus Ltd.<sup>3</sup>

A knowledge of the nature of linear programming and dual problems, their methods of solution, and the underlying assumptions, is presupposed.

## I—Dual prices for factor inputs

The discussion will be based on the following example.

<sup>1</sup>See, for example, Arnold [2], Carsberg [5] and [6], and Sal-kin and Kornbluth [8].

<sup>2</sup>See Amey [1], chapter IX, for an example of a detailed planning model.

<sup>3</sup>See Carsberg [5], chapter 11.

### *The Example of Gamma Ltd*

Gamma Ltd is considering two products for the coming year and the following data are available:

	<i>Product X</i>			<i>Product Y</i>		
	(Input required)	£	£	(Input required)	£	£
Selling price per unit			7.00			8.00
Less avoidable direct costs:						
Material M @ £2.00 per kilo	(1 kilo)	2.00		(2 kilos)	4.00	
Skilled labour @ £1.50 per hour	(2 hours)	3.00		(1 hour)	1.50	
		—	5.00		—	5.50
			—			—
Contribution per unit			2.00			2.50
			—			—
Inputs expected to be available during the year:						
Material M			350,000 kilos			
Skilled labour			400,000 hours			
Fixed overheads for the year			£300,000			

For ease of exposition, the 000s will henceforth be ignored. The problem can be formulated as a linear programming model thus:

$$\begin{array}{lll} \text{Maximise} & 2.00X + 2.50Y \\ \text{Subject to} & X + 2Y \leq 350 \\ & 2X + Y \leq 400 \\ & X, Y \geq 0 \end{array}$$

Fixed overheads, being constant, are irrelevant.

The final simplex tableau obtained when solving the problem is as follows:

	X	Y	S <sub>1</sub>	S <sub>2</sub>	
X	1	0	$-\frac{1}{3}$	$\frac{2}{3}$	150
Y	0	1	$\frac{2}{3}$	$-\frac{1}{3}$	100
	0	0	1	$\frac{1}{2}$	550

	Product X			Product Y		
	(Input required)	£	£	(Input required)	£	£
Selling price			7.00			8.00
Less:						
Material M @ £3 per kilo	(1 kilo)	3.00		(2 kilos)	6.00	
Skilled labour @ £2 per hour	(2 hours)	4.00		(1 hour)	2.00	
		—	7.00		—	8.00
Surplus per unit		—	—		—	—

This implies the following solution to which have been added some relevant sensitivity data:

#### Optimal plan

Produce and sell      150 units of X  
                                 100 units of Y

Contribution will be £550 (giving a net profit of £250)

#### Dual prices

Material M      £1.00 per kilo for up to a further 450 kilos  
Skilled labour   £0.50 per hour for up to a further 300 hours

The dual price for material M gives the value to the company of the marginal unit (of which more will be said in the next section) but, because linear programming assumes fixed input and output prices per unit and constant returns to scale in production, marginal and average values will be the same throughout the range considered in the problem. Consequently, the dual price gives the excess value to be placed on *each* of the 350 kilos that are expected to become available. A

similar interpretation can be given to the dual price for skilled labour.

If the directors of Gamma accept the optimal plan indicated above, having satisfied themselves that the assumptions of linear programming are an acceptable approximation of the market and technical situations the company faces, at least within the range considered, they may, as the literature suggests, make use of the dual prices for decision-making and management accounting purposes during the year. For internal purposes, each kilo of material M will be costed at £3 and each hour of skilled labour at £2. This will impute the whole of the contribution of £550 to the two inputs that impose effective constraints<sup>4</sup> so that each unit of X and Y will show a surplus of zero, thus:

Any product considered, but rejected, by the linear programme will show a deficit when costed at these rates. This result will be returned to later.

#### Interpretation of the Dual Price for Material M

The dual price for material M indicates that, if Gamma could acquire an extra kilo at the market price of £2, optimal reallocation of the two inputs (plus, in the general case, the bringing in of unused quantities of any inputs that do not impose effective constraints) would give a revised production plan, still based on production of both X and Y, showing a contribution of £1 more.<sup>5</sup> Conversely, if deprived of a kilo, the maximum possible contribution of the company would fall by £1. The sensitivity analysis indicates that the dual price remains unchanged provided not more

<sup>4</sup>i.e., £(350 × 1) + £(400 × 0.50) = £550.

<sup>5</sup>As the first two elements of the S<sub>1</sub> vector in the final simplex tableau imply, this would be achieved by producing a further  $\frac{2}{3}$  unit of Y and reducing production of X by  $\frac{1}{3}$  unit i.e., £( $\frac{2}{3} \times 2.50$ ) - £( $\frac{1}{3} \times 2.00$ ) = £1.

than an additional 450 kilos is obtained.<sup>6</sup> A naïve approach might suggest that the company could increase its contribution by offering up to £3 per kilo for a further 450 kilos.

On the assumptions built into the model this is true, but is it realistic? Positive economic analysis must now be brought in. Gamma has estimated that, during the coming year, only 350 kilos will be available on the market at £2 each; it would like more and, on the assumption that it wishes to increase profit, it will be prepared to pay a premium for further quantities. Assuming for simplicity that there are no substitutes, it should actively seek to acquire extra units. If there are no other changes in the values of the variables used in the model, the actions of firms like Gamma are likely to have repercussions in the market for material M.

If this is the case, and if the total supply available in the planning period is definitely fixed and has been correctly estimated by Gamma, then (in the absence of price controls) the market price for *all* units is likely to rise thus reducing the company's contribution for the year of £550. As the price increases towards £3 per kilo, the dual price will fall correspondingly. Using the data supplied, the production plan that is optimal for the company will be unchanged provided the market price does not exceed £3.<sup>7</sup> Over this price it can be shown that Y is the product that drops out of Gamma's plan because it uses more material M than X does.

However, the original estimate of the constraint may be wrong. The excess demand for material M may bring forth more supplies on to the market (e.g. by overtime working or by imports) and it may encourage the manufacture and use of substitutes. If extra supplies can be bought at less than £3 per kilo within the planning period, Gamma's contribution can be increased by diverting some labour from the production of X in favour of increasing the output of Y (because X uses more labour relative to the now more readily available material M than Y does). If more than an extra 450 kilos is likely to be made available (perhaps

as the result of a special effort by Gamma's buyer), the optimal range of products will change and the revised programme will show that the company should cease all production of X.

In reality, it is likely that a combination of events will often take place so that both the market price and the quantity on offer will increase within the planning period towards a situation where demand is equated with supply at a stable price. It is not unreasonable, however, to expect that there will be a time lag before any adjustment takes place, the delay varying according to the technological conditions of production of the material, the degree of imperfection in the market pricing mechanism for it, and other market conditions.

#### *Interpretation of the Dual Price for Skilled Labour*

Similar arguments may be applied to the dual price of 50p per hour for skilled labour (and, indeed, for any other inputs that need to be obtained during the planning period). It may be explained as the maximum the company should be willing to pay per hour as an overtime premium to induce hourly paid employees to work up to a further 300 hours.<sup>8</sup> It would, perhaps, not be surprising if the workforce were to assume that their rate of pay should be increased for *all* hours worked (especially if they learned that all labour hours were costed in the books at £2 each). They would, however, be wrong to think that 50p is necessarily an hourly increase that the company could pay in the longer run. This is because the dual prices impute the whole of the contribution to the scarce inputs: if all factors of production were to be paid their dual prices, the firm would make an accounting loss to the extent of its fixed overheads<sup>9</sup> indicating possible long-run unprofitability.

A dual price is not market-determined. From one aspect, the ascertainment of dual prices can be viewed simply as an accounting device that divides, by means of a particular algorithm, the planned contribution from output among the inputs that impose effective constraints, thereby placing a value on the short-run net revenue productivity per unit of each scarce input. Thus, a dual price sets a limit, for example, on the wage increase that could be offered in the short-run

<sup>6</sup>This figure can easily be checked. For each additional kilo of material M, the final simplex tableau shows that  $\frac{1}{3}$  unit less of X should be produced. As the optimal plan includes 150 units of X, it will drop out of the plan if more than 450 extra kilos of material M are acquired; a different basic solution will then be optimal so new dual prices will apply.

<sup>7</sup>The company will, in fact, make a net accounting loss for the year if the price for all units should rise above £2  $\frac{2}{3}$  (i.e., £ $\frac{2}{3}$   $\times$  350 = £250 = net profit), in which case its shareholders may be better off if it changes its line of business or goes into liquidation, unless this state is only temporary.

<sup>8</sup>The  $S_2$  vector of the final simplex tableau shows that, for each additional hour of skilled labour, production of Y will fall by  $\frac{1}{3}$  unit. Therefore, when more than 300 further hours are acquired, Y will drop out of the plan, a new basic solution will be optimal and new dual prices will apply.

<sup>9</sup>See footnote 4.

without altering the optimal plan. However, the *actual* wage increase offered (if any) will normally be market-related and take into account the local wage rate for labour with comparable skills (though, for some special skills, a standard for comparison may be difficult to agree upon).

#### *'Lump Sum' Costs*

Gamma may, on the other hand, wish to try to recruit additional employees to work the extra 300 hours and this may require expenditure on advertising outside the locality, or on introducing a training scheme. As far as the coming year is concerned, provided the cost (exclusive of the standard wage cost of £1.50 per hour paid to the new employees) does not exceed £150,<sup>10</sup> the additional expenditure is worthwhile. However, the company will need to think further ahead because of the possibility (and cost) of having to make superfluous staff redundant should this be necessary at a later date. It will need to estimate the potential cash flows for a number of years ahead and, using normal investment criteria that take cost of capital into account, assess whether the expenditure is likely to enhance the company's returns over the longer term.

Similar considerations would apply to other inputs that imposed a constraint on production but which might not be available in small units or for short periods of time such as factory, warehouse and office space, machine capacity and salaried staff.

#### *Conclusion to Part I*

The tendency in the literature is to emphasise the rôle of the sensitivity data in assessing the effect on results should estimates turn out to be incorrect due to the inherent uncertainties in making accurate forecasts. The main point being stressed here, however, is that the very existence of dual prices may be a transitory phenomenon. They arise because of expected disequilibrium situations which may not take very long to adjust themselves. Thus, in adopting the linear programming approach to product selection, it is important to take careful account of the length of time during which the assumed rationing of inputs is likely to hold, and of possible changes in the market for each rationed factor. This is particularly true if dual prices are introduced into the formal management accounting system, e.g., as transfer

prices, because this may lead to increased inflexibility and non-optimisation.

## **II—Demand constraints**

The relevant literature on linear programming is mostly concerned with input constraints and there are few examples which incorporate demand constraints. A notable exception is Carsberg's example relating to Cygnus Ltd, the figures of which will be used here for illustration purposes. Details of the problem are given in the appendix to this paper; the reader may refer to Carsberg's book where the solution is derived and explained.<sup>11</sup>

#### *Imputing Profit to Scarce Resources*

As illustrated in the example of Gamma, contribution can be imputed to the scarce resources so that all products in the optimal set will show a surplus of zero when charged with the dual values of the scarce resources they employ. This is a feature which is made use of in the literature when discussing how dual prices can be applied in the areas of transfer pricing, decentralised decision-making and control, and in the screening of new products. For example, Baumol suggests that

The dual accounting prices... can serve as a device for steering decentralised decision-making along an optimal course... The perfect plant manager will just break even because the dual accounting prices are calculated so as to eat up all his profits; but they will cause no loss only if all his product-line decisions are optimal.<sup>12</sup>

Clearly, to get a surplus of zero for all acceptable products means that one must also charge the dual prices for demand, although the literature does not appear to consider this point. Just as men and machines are resources, so the market place can be looked on as a resource which is used up as any other. However, it is not essential to charge the demand duals provided one is careful in the interpretation of the surpluses per unit that remain if one chooses not to do so.

#### *Interpretation of the Dual Prices for Demand*

In Carsberg's example of Cygnus, six products were considered for manufacture and, in the solu-

<sup>11</sup>See Carsberg [5], 165–181.

<sup>12</sup>Baumol [4], 115–116. See also Arnold [2], Salkin and Kornbluth [8], Samuels [9] and Solomons [10]. For a critique of some of the uses of dual prices in management accounting, see Barron [3].

<sup>10</sup>i.e., 300 hours at the dual price of 50p per hour.



tion, they were left with surpluses (i.e., dual prices for demand) and deficits as follows:

Product

A	+ £14.10 per unit
B	0 per unit
C	0 per unit
D	+ £ 5.80 per unit
E	+ £12.70 per unit
F	- £ 1.40 per unit

The interpretation of dual prices for demand is basically no different from that for factor inputs except that the former relate to one product only (in the case of products whose demand is independent) whereas inputs are often used in two or more products. Taking A as an example, the data in the question assumed that, at a selling price of £67 per unit, the market would be willing to buy up to 800 units. Assuming no other changes in the data, the dual price for its demand indicates:

- (i) that, by reallocating some units of the inputs that impose effective constraints, the company will be better off by £14.10 if the market were to take 801 units of A, and worse off by the same amount if it took only 799 units.
- (ii) the sensitivity of the contribution of A, i.e., if its selling price were reduced by up to £14.10 (or an element of cost peculiar to A rose by this amount per unit of A), the optimal production plan would be unchanged.

B and C have dual prices for demand of zero because, although they feature in the optimal plan, there is excess demand at the selling prices used in the model. F has a deficit which indicates that it does not form part of the optimal plan and so should not be produced.

Following the type of argument that is sometimes adopted in the literature, it may be suggested that, as it has the greatest surplus per unit, selling effort should be concentrated on A in order to persuade the market to take more than 800 units at £67. However, the following points should be noted:

- (i) On the basis of the above figures alone, it is worthwhile trying to increase demand for all three products that show a surplus. To concentrate on only one would appear to be irrational in the absence of any other unspecified constraints.
- (ii) Before deciding where to concentrate any additional selling effort, one needs to estimate the demand conditions for each of the five acceptable products. It was initially assumed, for example, that the market would take 800 units

of A at £67 each but, despite the salesmen's efforts, it may not take any more units at the same price.

(iii) There may well be additional costs (of a per unit or 'lump sum' nature) incurred in the extra sales effort and these may differ between the products. Combined with the possibility that the extra units (or perhaps all units) can only be sold at a reduced price, it may not even be profitable to promote further sales of any product.

### *The Worthwhileness of a Product*

In a professional examination a few years ago, a question gave cost and dual price data relating to two of the products included in a hypothetical company's linear programme for planning purposes.<sup>13</sup> Candidates were asked, *inter alia*, to calculate minimum prices at which sale of the two products would be worthwhile (which is here assumed to mean profitable) according to the linear programme. No mention was made of demand constraints, nor of dual prices for demand, and no information relating to the sensitivity of the variables in the solution was given. If the data had been the same as for Cygnus's products A and F, the following calculations could have been made:

	A £	F £
Selling price per unit	67.00	35.00
Less avoidable costs, in total	40.50	27.00
Contribution per unit	26.50	8.00
Less dual values of inputs, in total	12.40	9.40
Surplus (deficit) per unit	14.10	(1.40)

As will be shown shortly, the question cannot be fully answered as regards A with the limited data supplied. (The examiners may, perhaps, have wished candidates to point this out and to make their own assumptions.)

The question can be answered as regards F; if F's price were increased by more than £1.40 per unit, and no other changes took place, the profit of the company could be increased by its production provided the market were willing to buy some units at the higher price. At £36.40 per unit, the company would (on short-run profit grounds) be indifferent between implementing the existing optimal plan and the one which included

<sup>13</sup>The Certificate in Management Information and Part I, Joint Diploma in Management Accounting Services, paper entitled "Financial Management and the Use of Accounting Information", October 1972, question 4.

F. Thus, the minimum price at which sale of F can be said to be worthwhile is £36.41 (assuming fractions of pence are not permitted).

Product A shows a surplus and, if the data are otherwise complete, £14.10 must be the dual price for the market demand for it. One can say that, if the price is reduced by not more than this amount (i.e., to not below £52.90 per unit) and there are no other alterations to the original data (including no change in F's price), the optimal plan will be unchanged. But, whether a price below £67 is worthwhile depends on the elasticity of the demand curve for A on which no information is supplied.

Here, for a realistic solution, positive economics must again be brought in. If the demand curve for A is perfectly inelastic (which is implied by varying only the selling price and not the quantity demanded), the firm will be giving away profit unnecessarily because, if it is able to sell 800 units at £67 each, there is no point in reducing the selling price in order to sell the same quantity. In such circumstances, any price below £67 can hardly be said to be worthwhile. If, however, market demand for A increases in response to a reduction in its price, a lower price will be worthwhile if it causes the company to earn a higher total profit than it would by keeping the price at £67.

It is well known that dual prices give useful information if the estimates are subject to uncertainty. If the market price at which 800 units can be sold has been misjudged, the figures tell us that the optimal plan will remain unchanged providing the price does not fall below £52.90 and there are no other changes in the data. This does not, however, necessarily give the minimum price at which sale of any units of the product is worthwhile. As the selling price of a constrained product goes down, and a new basic solution becomes optimal, it may still be profitable to produce smaller quantities of it, the changes taking place in discrete steps.

From a sensitivity analysis it can be shown, for example, that production by Cygnus of some units of A is, in fact, worthwhile provided the market price for up to 800 units does not fall as low as £44.50 per unit, viz:

Selling price of A per unit	Quantity to sell (maximum 800 units)
£	units
52.90 and above	800
46.50–52.89	566 $\frac{2}{3}$
44.51–46.49	400
0–44.50	0

### Conclusion to Part II

The comments in the first part of this paper apply also to the interpretation of dual prices in respect of demand constraints: i.e., the initial assumptions of linearity must be questioned at this stage and estimates of price elasticity, and of the effect of these on the optimal plan, introduced and linked to the sensitivity analysis.

In addition, care has to be taken in interpreting the surpluses that remain on products simply because the dual prices for market demand have not been charged to them. This has particular relevance to the use of dual prices in decision-making situations such as where to concentrate additional selling effort, and in decentralised decision-making where the existence of surpluses renders invalid the 'break even' instruction. Indeed, if dual prices in respect of factors of production are considered in decision-making (and maybe incorporated in a company's management accounting system),<sup>14</sup> there seems to be no logical reason for not treating dual prices for demand in a similar manner.

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### Appendix

#### *The Example of Cygnus Ltd*<sup>15</sup>

Cygnus Ltd uses three resources that are subject to constraints: labour of three grades, skilled, un-

<sup>14</sup>See Demski [7] and Samuels [9] for suggested management accounting control systems.

<sup>15</sup>See Carsberg [5], 165–181, where the solution to the problem is derived and explained.

skilled and semi-skilled, with wage costs of £1, £0.50 and £0.75 per hour. Maximum available supplies for the coming year are estimated at 16,000 hours each of skilled and unskilled labour and 12,000 hours of semi-skilled labour. No transfers are possible between the different grades of labour. Fixed costs are £29,000 per annum. Details of the products being considered for manufacture are given in the accompanying table. The problem is described by the following mathematical model.

The company wishes to maximise:

$$26.5x_a + 10.5x_b + 15x_c + 19x_d + 28.5x_e + 8x_f,$$

subject to the constraints:

$$10x_a + 8x_b + 4x_c + 8x_d + 2x_e + 4x_f \leq 16,000$$

$$4x_a + 8x_b + 10x_c + 4x_d + 4x_e + 2x_f \leq 16,000$$

$$2x_a + 2x_b + 8x_c + 4x_d + 10x_e + 4x_f \leq 12,000$$

$$x_a \leq 800$$

$$x_b \leq 400$$

$$x_c \leq 1,000$$

$$x_d \leq 600$$

$$x_e \leq 500$$

$$x_f \leq 1,000$$

$$x_a, x_b, x_c, x_d, x_e, x_f \geq 0$$



# Price-Level Reporting and its Value to Investors

Philip C. Devon and Richard Kolodny

## Introduction

The question of whether the effect of price changes on the value of assets and liabilities should be reported in financial statements has been debated for more than half a century in the United States. As early as 1920, Paton [13] proposed that both historical cost and price-level adjusted data be reported. Since that time the accounting and investment professions have studied this question on several occasions.<sup>1</sup> In 1974, the Financial Accounting Standards Board (FASB) released an Exposure Draft [7] recommending that firms be required to disclose price adjusted data as a supplement to conventional financial statements. More recently the Securities and Exchange Commission became the dominant participant in the controversy. In its Accounting Series Release No 151 [16] of 1974, it urged registrants to make 'supplemental disclosure' of 'inventory profits', defined as the difference between historical cost of goods sold and the replacement cost of such goods at the time of sale. More importantly, in 1976 the SEC asked that large companies report supplementary information on fixed asset and inventory replacement cost plus depreciation and cost of sales based on such values [17]. Because the SEC called for this alternative form of 'price level accounting', FASB deferred its recommendation that financial statements be adjusted for changes in the general purchasing power of money.

In Britain, an accounting system proposed by the Sandilands Committee [15] which would require disclosure of current cost values is being reviewed. Also, Australia is considering a proposal that would require companies to adopt current cost accounting.

<sup>1</sup>For instance, the American Institute of Certified Public Accountants' Committee on Accounting Procedure considered the problem in 1947 [2], 1948 [4], and 1953 [3]; its Accounting Research Division published ARS No. 6 [5] on the subject in 1963; and the Accounting Principles Board made proposals regarding the issue in APB Statement No. 3 [1] in 1969.

The debate over whether or not firms should be required to report price-level adjusted data, and if so, what the nature of that requirement should be has drawn renewed attention partly because of the high levels of inflation in recent years and the prospect that relatively high levels will persist in the future.

This study focuses on a particular form of accounting for changing prices—that recommended in the FASB Exposure Draft of December 1974. Although professional opinion generally seems to have moved on from the form of general index accounting of the FASB exposure draft, there is still debate about whether general price-level effects should be reported *as well as* specific price movements. Moreover, research into 'pure' general index accounting may shed light on reporting for price movements using more detailed procedures.

In evaluating the need for price-level data, two questions are of particular interest to both financial analysts and the accounting profession. First: How large an impact will adjusting data for price-level changes have on financial statements? If, in fact, the impact is minimal, or the same for all firms, compiling and reporting such data may not warrant the cost incurred. Secondly, even if the impact is significant, this, in itself, does not indicate that requiring its disclosure is justified. Before reaching a conclusion regarding this issue, a second question must be answered: What meaning and value will price-level adjustments have to users of financial data? For, whatever justification there is for requiring the disclosure of price-level data must ultimately lie in its usefulness to investors and creditors.

Regarding the first question, Sidney Davidson and Roman Weil [6] reported the results of a comprehensive study in which they examined the effects of adjusting reported earnings for general price-level movements. Using companies comprising the Dow Jones Industrial Average and 30

others represented in the *Fortune* 500, they illustrated how adjustments may be made, and more importantly, evaluated the effect which adjustments had on 1973 reported income. Their results indicate the existence of an extremely important inflationary impact on earnings. Moreover, their results show a substantial difference in the effects which adjustments have among firms:

The results demonstrated clearly the error of the frequently stated naive view that all firms are affected relatively equally by inflation, and that a single overall adjustment factor applied to the reported profit of all firms will yield satisfactory results. Adjusted net income as a percentage of reported net income ranges from 18 to 153% for the Dow 30 and from 36 to 176% for the other 30.<sup>2</sup>

Davidson and Weil thus present strong evidence that price-level adjustments to firms' revenues and expenses lead to significantly different reported earnings; and that the effects of these adjustments vary widely among firms.

The purpose of this study is to furnish information pertaining to the second question. Davidson and Weil have provided evidence on the extent of the difference and variability between adjusted and conventionally reported figures. However, the value of such information to users of financial reports has been merely conjecture. It is the authors' purpose to evaluate what benefits, if any, could have been obtained had price-level information been available to investors.

This problem is approached by assessing the relationship between a firm's earnings, both on an adjusted and an unadjusted basis, and the evaluation of the firm's securities. It is assumed that if a strong relationship exists between security returns and price-level adjusted data, then this data could be of value in investment analysis. As is discussed below, previous studies have shown that unadjusted earnings figures are important to investors. Therefore, the authors use the relationship between unadjusted data and security prices as a standard to measure the potential benefits of adjusted data.

To determine the above relationships, two statistical tests were conducted. The precedent for the first was established by Niederhoffer and Regan [12]. The second was designed solely for the purpose of this research. Before reviewing these tests, the reasons why adjusted data are potentially relevant to investor decisions are considered.

### The case for reporting price-level earnings

The objective of the preparation of financial statements, as defined in APB Accounting Principles, is to provide information that will aid the firm's owners and creditors in making economic decisions.<sup>3</sup> Perhaps the most important single piece of information contained in financial statements which bears upon investor decisions is the earnings generated by the firm. Evidence of this is found in a report on the objectives of financial reporting prepared by the Financial Analysts Federation.<sup>4</sup> This report identifies a firm's 'earning power', defined as the 'ability of the company to produce continuing earnings from the operating assets of the business...'<sup>5</sup> as a major concern of financial analysts. Moreover, the results of a number of empirical studies indicate a strong association between a firm's earnings and the price of its equity securities; and that accurate predictions of future earnings are extremely helpful in investment decision making.<sup>6</sup>

Earnings and other data reported in financial statements will be most indicative of future 'earning power' and therefore of greatest value to investors when they reflect, as closely as possible, economic reality. As recognized by the Financial Analysts Federation, 'If financial markets are efficient, economic reality will be reflected in those markets and, therefore, analysts need financial statements which accord with economic reality.'<sup>7</sup> It may be argued that (1) when the purchasing power of the dollar changes, price-level financial information will reflect more accurately the economic consequences of business operations than will historical-dollar information, and (2) as the magnitude of inflation increases, the information provided by conventional statements loses significance.

Underlying the above contentions is the fact that transactions reported in conventional statements occur at different points in time, and thus, the dollar units which record these transactions may not be equivalent in terms of purchasing power. For example, whereas revenues are valued in current or near-current dollars, expense items such as depreciation and cost of goods sold may be valued in dollars of earlier periods. In times of increasing prices, therefore, the income which

<sup>2</sup>Davidson and Weil [6], p. 28.

<sup>3</sup>APB Accounting Principles, Section 1024.01.

<sup>4</sup>'Objectives of Financial Accounting and Reporting from the Viewpoint of the Financial Analyst' [8].

<sup>5</sup>*Ibid.*, p. 13.

<sup>6</sup>Several studies which reach these conclusions are summarised in Lev [11], pp. 277-238.

<sup>7</sup>'Objectives of Financial Accounting,' *loc. cit.*, p. 10.

is reported represents in part inflationary income which exists merely on paper. Such income does not represent the economic concept of earnings, i.e., the increase in the firm's real worth, as measured by its capacity to buy goods and services. Also, because under inflationary conditions the original costs used in computing income will tend to be lower than the costs of replacing extinguished assets, if dividends were paid in the amount of the reported income, the firm would be unable to replace the sold units. Hence, reported income cannot be considered a 'profit' in real terms. Finally, because the amount, composition, and age of assets and liabilities vary across firms, the effects of inflation differ among firms, and consequently, the use of historical-dollar figures impairs inter-firm comparisons of performance.

Most analysts recognise to some degree the problems associated with historical data in general, and historical earnings in particular. The current controversy centres around what positive effects, if any, would arise from the publication of adjusted financial data. Supporters of this requirement generally believe that, although price level accounting will result in imperfect adjustments, the earnings figure will better represent the value added by the firm during a period than those based on historical cost earnings. Therefore, they argue that to provide earnings data on a historical basis only is to provide an incomplete and less useful set of information. Those who oppose the publication of adjusted earnings data, on the other hand, contend that such data will not inform, but may confuse, users of financial statements.<sup>8</sup>

### Market tests

To evaluate whether knowledge of earnings data adjusted for price-level changes are of any net benefit to investors over conventional reported data, a sample initially comprising the largest 250 companies listed in the May 1974 issue of *Fortune* magazine was selected. Because required test data were unavailable for some companies, the sample size was later reduced to 184. For each of these companies, financial data was obtained from the COMPUSTAT Annual Industrial File for the years 1964–1973. Historical earnings were restated in each of the ten years by using a procedure in general conformity with the FASB Exposure Draft of December 1974 and similar to that detailed in Davidson and Weil [6]. Essential

elements of this procedure are summarised below.

Reported depreciation expense for each year was estimated by deducting beginning accumulated depreciation from the same account at the end of the period. The average age of fixed assets then was determined by comparing the depreciation expense with the accumulated depreciation account at the end of the period. To obtain adjusted depreciation, the estimated reported figure was multiplied by the adjustment factor, i.e., the ratio of the current Gross National Product Deflator to the Deflator in the estimated average year of asset purchase. Revenues, income taxes, and other expenses were treated as though they occurred evenly throughout the year. The monetary gain or loss was determined by averaging each 'monetary' account (cash, accounts receivable, current liabilities, long term debt, and preferred stock), obtaining a net asset amount, and applying the inflation rate during the period, i.e., multiplying the net asset amount by 1—the adjustment factor for that period. The price-level earnings per share (hereafter, P-EPS) for each year was then computed as follows:

$$\text{P-EPS} = (\text{Adjusted Net Income/Loss Before Depreciation Expense} - \text{Adjusted Depreciation Expense} \pm \text{Monetary Gain or Loss} / \text{Number of Common Shares Outstanding})$$

Using the derived P-EPS, two tests were performed to examine whether changes in security prices have been more closely related to the change in historical dollar earnings per share (hereinafter, H-EPS) or to the change in P-EPS. The first of these was based on tests presented by Niederhoffer and Regan [12]. Niederhoffer and Regan offer empirical evidence which supports the hypothesis that stock price fluctuations are dependent on earnings changes. Using similar methodology and the same time period as in that study, the authors tested whether market performance as measured by the percentage change in stock price was associated more highly with P-EPS or H-EPS. The procedure was to compute the percentage change in stock price in 1970 for each of the 184 firms in the sample. Based on these changes, the 30 best and the 30 worst performers were identified, and for each, the 1969–1970 changes in both P-EPS per dollar of stock price and H-EPS per dollar of stock price were calculated. Tables 1 and 2 show the distribution of changes in earnings per dollar of stock price for the 60 securities; Table 1 uses price level adjusted earnings and Table 2, unadjusted. Examination of Tables 1 and 2 reveals two phenomena.

<sup>8</sup>Accounting Research Bulletin No. 33 [2], p. 8.

**Table 1**  
**Distribution of P-EPS Changes per Dollar of Stock Price**

<i>Earnings per Share Changes (Cents per \$ of Stock Price)</i>	<i>Top 30</i>	<i>Bottom 30</i>
18 and over	1	
16 to 18		
14 to 16		
12 to 14		
10 to 12		
8 to 10	1	1
6 to 8		
4 to 6	1	
2 to 4	6	
0 to 2	9	3
-0 to -2	9	12
-2 to -4	2	4
-4 to -6	1	2
-6 to -8		1
-8 to -10		
-10 to -12		1
-12 to -14		1
-14 to -16		
-16 to -18		1
-18 and over		4

First, the group of best market performers in both tables manifested relatively higher positive change in earnings than the group of worst performers. For instance, as Table 1 indicates, using price level adjusted data, positive earnings per share changes were achieved by 18 of the top 30 market performers compared to only 4 of the worst; while only 3 of the top performers had earnings changes per dollar of stock price less than 0.02 compared to 14 in the bottom 30.<sup>9</sup> These data thus confirm the findings of Niederhoffer and Regan: a direct relationship appears to exist between earnings changes and market performance.

The other phenomenon revealed in Tables 1 and 2 is a somewhat stronger relationship between price adjusted earnings and performance than between unadjusted earnings and performance. For instance, comparing the first column of Table 1 with that of Table 2 indicates that whereas 18 of the best performing stocks had positive changes in adjusted earnings per share, only 13 had positive changes in unadjusted earnings per share. A comparison of the worst performers shows that 26 had negative changes in P-EPS while 24 had negative changes in H-EPS.

<sup>9</sup>Similar but greater distinctions were found between similar groups by Niederhoffer and Regan [12]. This is to be expected, however, since the best and worst performers in their study were drawn from a sample more than six times the size of the authors'.

These results suggest that changes in price-level earnings are more strongly related to market performance than are historical-dollar earnings.

The second test performed examined the relationship between earnings and security prices over the ten-year period 1964-1973. For this period the compound growth rate in H-EPS and the percentage change in common stock prices were computed for each of the sample companies. The average earnings growth rate, 0.0637, was used to divide the 184 companies into two groups: (I) companies whose earnings growth rate exceeded the average, and (II) companies whose growth rate in earnings was below the average. For both groups the average percentage change in common stock price over the ten-year period was determined. In addition, groups I and II were each divided into two equal subgroups: (A) firms which were affected most favorably by price-level changes—price-level gainers, and (B) firms which were affected least favorably—price-level losers.<sup>10</sup> Average security price changes were calculated for each of these subgroups. The results of the analysis are shown in Table 3.

An examination of rows (1) and (4) provides further support for the premise that there is a

<sup>10</sup>The purpose of first partitioning the sample on the basis of unadjusted earnings and then forming subgroups (A) and (B) was to attempt to separate out the additional price level effect, after adjusting for the effects of historical earnings growth.

**Table 2**  
**Distribution of H-EPS Changes per Dollar of Stock Price**

<i>Earnings per Share Changes (Cents per \$ of Stock Price)</i>	<i>Top 30</i>	<i>Bottom 30</i>
18 and over	1	
16 to 18		
14 to 16		
12 to 14		
10 to 12		
8 to 10		
6 to 8		
4 to 6		1
2 to 4	3	
0 to 2	9	5
-0 to -2	12	13
-2 to -4	4	3
-4 to -6		2
-6 to -8	1	
-8 to -10		
-10 to -12		
-12 to -14		1
-14 to -16		
-16 to -18		
-18 and over		5



**Table 3**  
**Relationship Between Earnings Growth and Changes in Security Prices From 1964-1973**

		(1) <i>Relationship to Average Growth in Unadjusted Earnings</i>	(2) <i>Price Level Gainer/Loser</i>	(3) <i>Percentage Change in Market Price</i>	(4) <i>Percentage Average Difference Between P-EPS and H-EPS</i>
(1)	I	Above	Total Gainers and Losers	270%	
(2)	IA	Above	Gainers	220%	[256]* 15%
(3)	IB	Above	Losers	320%	[221]* -9%
(4)	II	Below	Total Gainers and Losers	47%	
(5)	IIA	Below	Gainers	63%	16%
(6)	IIB	Below	Losers	31%	-12%

\*Numbers in brackets represent percentage changes when outliers were eliminated from the sample; see text for further explanation.

direct relationship between earnings growth and changes in security prices. The percentage change in market price over the 10-year period for those companies with relatively high growth in earnings averages 270% while for those with relatively low growth in earnings, the change was 47%. This result is consistent with those reported above. With respect to price-level data, if changes in P-EPS are reflected in security price movements to a greater extent than their historical counterparts, the performance of price-level gainers in group I should exceed the performance of price-level losers in the same group. Similarly, price-level gainers in group II should exhibit relatively better market performance than losers. The data in Table 3 support these relationships. Price-level gainers outperformed losers in group II. More specifically, it is shown that knowledge of price adjusted earnings would have been extremely beneficial for investing in companies with below average earnings growth. With this information, an investor could have received more than twice as great a return by selecting price-level gainers rather than losers (63% as opposed to 31%).

The benefits of using price-level information to select among firms with rapidly growing earnings are less clearly defined. As indicated by the first set of numbers in column 3, the utilisation of price-level information to select among those firms with rapidly growing earnings would have provided no benefit. However, a review of the firms in group I revealed the presence of a few high-growth, high-multiple companies whose percentage changes in market price were significantly different from those of other sample companies. The elimination of these companies would reverse the relative market price percentages as shown

by the bracketed numbers in column three, i.e., excluding those companies, group IA and IB showed percentage changes in market prices of 256% and 221%, respectively. Thus, it appears that a smaller level of importance could be attributed to price-level data for fast growing firms. The failure of price level information to clearly differentiate between better market performance in group I may also be explained partially by the fact that the difference between H-EPS and P-EPS for group I is smaller than it is for group II.

### Concluding remarks

The results of this study generally lend support to the position that supplemental price-level data should be required. Although all the empirical results do not uniformly demonstrate that price-level information is significantly more useful to investors than is historical information, they do suggest that price-level data can be of value. The results presented in Tables 1, 2 and 3 provide evidence that the recognition of the effects of inflation can be useful in discriminating between 'good' and 'poor' market performers.

In addition, because the effects of inflation vary considerably across firms, the value of price-level information and its interpretation will be different for each firm. Thus, although the authors' tests were performed with aggregate data, it is important to emphasise that the use of price-level information must be considered on the basis of the individual firm and its particular characteristics. Most probably, price-level information is more valuable for analysing the securities of some firms than for others. Because the authors' tests were on an aggregate basis, therefore, the benefits that

might accrue with regard to individual securities may have tended to be concealed.

Finally, it is evident that inflation was mild during the period studied relative to recent experience, and, therefore, the impact of inflation on security prices today and in the future may be significantly more pronounced than for the period tested.

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# Does Accounting Research Matter?

Scott Henderson and Graham Peirson

It is frequently asserted that, other things being equal, individuals are happier if they believe that their work 'matters'. There is no reason to doubt that this assertion is appropriate for accounting researchers who do not engage in research for its own sake but because they believe that it matters. Of course, 'mattering' may be interpreted in a number of ways. At the personal level there is little doubt that accounting research matters. It matters to graduate students in accounting, because their research may enable them to complete the requirements for a higher degree. It matters to teachers of accounting for two reasons. First, publication of the results of their research improves their promotion prospects and second, the results of research may be used to improve the content of their lectures. Accounting research matters to research assistants and others paid to do nothing but research because it is their livelihood. It matters to University accounting departments because their reputation is largely dependent upon the quantity and quality of members' research and publications.

Another interpretation of mattering, however, is that accounting research matters only if it has an effect on accounting practice. A number of writers have suggested that in this context accounting research has not mattered. For example, commenting on US experience, Stephen Zeff suggests 'that the academic literature has had remarkably little impact... upon the policies of the American Institute or the SEC' [1]. This implies, perhaps, that accounting practice would not be very different from its present form if there had been no academic accounting research.

This paper considers the hypothesis that accounting research has had no effect on practice.

The paper is divided into three sections. The first explains our interpretation of accounting research. The second surveys the accounting research of the past 500 years and the third considers the impact of this research on accounting practice.

## The nature of accounting research

Accounting research may be defined as the testing of accounting related hypotheses. The first, and probably the most important, step in the research process is the choice of a hypothesis for testing. The ideas for hypotheses may be generated in a number of ways. Some may be the result of casual observation. For example, observation of accounting practice may suggest to a researcher that accountants only change the method of accounting for depreciation in order to 'smooth' reported profit. Others may be the result of reviewing the accounting literature. For example, reading the accounting literature of the 1920s and the 1930s may suggest the hypothesis that the profit and loss statement replaced the balance sheet as the focus of reporting practice. A few hypotheses may be formulated as the result of hunches. For example, it may occur to a researcher that cash flow statements would provide information more relevant to users than conventional financial statements.

The choice of a hypothesis is critical. In many respects this aspect of research requires the most imagination and skill. By comparison, the subsequent testing of the hypothesis is often a fairly mechanical process. The essential characteristics of a 'good' hypothesis are that it should be both plausible and testable. An implausible hypothesis is hardly worth testing and the time devoted to the research is likely to be wasted. For example, a hypothesis that double-entry bookkeeping originated in Australia is so implausible that it is not worth testing. Similarly, a hypothesis which cannot be tested is hardly worth formulating. For example, a hypothesis that current cost accounting is in widespread use on Mars may be plausible but it cannot be tested.

Hypotheses are of different types. Some merely describe accounting practice. For example, a descriptive hypothesis would be that accountants use the straight-line method of depreciation more

frequently than the reducing-balance method. To test this hypothesis a researcher will observe what accountants do. To this end he may ask company accountants which method of depreciation they use and/or he may conduct a survey of published company reports. Tests involving the observation of accounting phenomena are usually labelled 'empirical' and for this reason descriptive hypotheses are sometimes referred to as empirical hypotheses. If empirical research supports a hypothesis, it is said to be *confirmed* and labelled a theory.

Other hypotheses are formal. They are concerned with consistency between the statements comprising the hypothesis. A formal accounting hypothesis would be that given double-entry bookkeeping rules, assets equal liabilities plus proprietorship. Formal hypotheses are tested using mathematics, logic or some other set of rules. The test is concerned only with the internal consistency of the hypothesis. In the above example, bookkeeping rules together with logic will show that assets equal liabilities plus proprietorship. Where testing supports a formal hypothesis it is said to be *validated* and also labelled a theory.

In many instances, accounting researchers do not test single hypotheses. They test sets of hypotheses. In some cases they test an explanatory set of hypotheses. For example, an explanatory set of hypotheses would be that accountants measure the historical cost of assets rather than their current cost because the former is more objective than the latter. This explanatory set consists of two descriptive hypotheses which could be tested by empiricism. These descriptive hypotheses are: (1) that accountants measure the historical cost of assets rather than their current cost, and (2) that the former is more objective than the latter. The link between these descriptive hypotheses is indicated by the word 'because'. This link is a formal hypothesis which can be validated. Explanatory sets of hypotheses explain behaviour or phenomena and to test them involves both empiricism and logic, mathematics or some other set of rules.

Other sets of hypotheses are prescriptive or normative, suggesting courses of action. A normative set of hypotheses would be that accountants should measure current cost rather than historical cost because the former is more useful than the latter. This normative set consists of a descriptive hypothesis and a formal hypothesis. The descriptive hypothesis is that current cost is more useful than historical cost and can be tested empirically.

**Table 1**

<i>The Nature of the Hypothesis(es)</i>	<i>The Nature of the Research</i>
Descriptive	Empirical
Formal	Mathematics, logic or some other set of rules
Explanatory	Empirical and mathematics, logic or some other set of rules
Normative	Empirical and mathematics, logic or some other set of rules

The formal hypothesis is that given that current cost is more useful than historical cost, and that usefulness is an agreed objective of accounting, accountants should measure the former rather than the latter. This formal hypothesis cannot be tested empirically, but it can be validated by logic. Testing and validating a normative set of hypotheses also involves both empiricism, and logic, mathematics or some other set of rules.

In some cases explanatory and normative sets of hypotheses are so large and complex that if confirmed or validated they would explain or prescribe the whole area of accounting.

In summary, then, research is hypothesis testing and the appropriate research procedure depends upon the nature of the hypotheses being tested. This relationship is summarised in Table 1.

### Accounting research

Prior to the year 1800 the double-entry bookkeeping system that had been developed in Italy during the 14th and 15th Centuries was being disseminated throughout Europe. By 1800 the Italian system of double-entry bookkeeping was firmly established in Europe. During the period between the publication of Pacioli's book in 1494 (reputedly the first published on double-entry bookkeeping) and 1800, books on bookkeeping concentrated on its mechanics. They listed the rules and procedures that had to be followed to prepare the accounts of a business, but no effort was made to explain or justify those rules and procedures. Peragallo has written that 'no theory of accounting was devised from the time of Paciolo down to the opening of the 19th century' [2]. It can be argued, therefore, that there was no accounting research prior to 1800, because a theory only results when a hypothesis is confirmed or validated by research.

**Table 2**  
**Stages in the Evolution of Accounting Theory Since 1800**

<i>Period</i>	<i>Nature of the Majority of Hypotheses being tested</i>	<i>Nature of Research</i>
1801–1955	Explanatory	Primarily empirical but some mathematics or logic
1956–1970	Normative	Primarily logic or mathematics but some empirical
1970–present	Descriptive	Empirical

The accounting research that has taken place since 1800 can be divided into three time periods. In each of these periods the majority of the research effort was devoted to testing similar types of hypotheses. From 1801 to 1955, researchers concentrated upon testing explanatory sets of hypotheses. Researchers were primarily interested in explaining and rationalising accounting practice. From 1956 to 1970, researchers concentrated upon testing normative sets of hypotheses. Researchers were primarily interested in prescribing what accountants ought to do. Since 1970, accounting research has generally been characterised by the testing of descriptive hypotheses. These stages in the evolution of accounting research are summarised in Table 2.

We will briefly survey the research of each period in order to assess its impact upon accounting practice.

### 1800–1955

Beginning in about 1800 writers on accounting attempted to explain the reasons underlying the detailed bookkeeping rules and procedures. Although this period began about 1800, it was not until the beginning of the 20th century that this phase in the development of accounting research began to flourish. The incentive to explain and justify accounting practice was provided by a desire to improve the quality of instruction in accounting. At the turn of the century, instruction in accounting was a matter of rote learning. Books on the subject listed detailed rules and procedures and provided numerous worked practical examples which were learnt by heart. A number of writers regarded this approach as unsatisfactory. They believed that if a few simple principles could be devised, students of accounting would be able to understand why

the procedures listed in these books were recommended.

One of the earliest hypotheses which was tested by observing accounting practice was the proprietorship or ownership hypothesis. This hypothesis proposed that the proprietor or owner of a business was the focus of accounting procedures. Assets were owned by the owner, liabilities were owed by the owner, revenues were received for the owner and expenses were paid for the owner. Transactions were interpreted from the owner's viewpoint. The proprietorship or capital account was a control account for all the other accounts in the ledger. The proprietorship hypothesis can be illustrated by the equation: [3]

$$\text{Assets} - \text{Liabilities} = \text{Proprietorship}$$

Any transaction which increased assets or decreased liabilities, increased proprietorship by an equal amount. Conversely, any transaction which decreased assets or increased liabilities, decreased proprietorship by an equal amount. The effects of all transactions were summarised by changes in the proprietorship account. The proprietorship hypothesis proved very useful as an explanation of why accounting rules such as 'for a cash sale, debit cash and credit sales' were appropriate. Making the proprietor's interest the focus of bookkeeping procedures revealed the logic and the reasonableness of the rules. By the end of the 19th century the hypothesis had acquired the status of a theory and had widespread support.

Another explanatory hypothesis of accounting was tested by observing accounting practice during the second half of the 19th century. This was the entity hypothesis. The rise of the company as an important form of organisation meant that the assumption about the close relationship between the owner and the business implicit in the proprietorship theory was less appropriate. The entity hypothesis made the business the focus of accounting. All transactions were interpreted from the viewpoint of the business. Assets belonged to the business rather than to the owner, while liabilities were owed by the business rather than the owner. Revenues were earned for the business and not for the owner, while expenses were incurred for the business and not for the owner. From the business's viewpoint the owner was a provider of resources in much the same way as a creditor. The entity hypothesis can be illustrated by the equation: [4]

$$\text{Assets} = \text{Liabilities} + \text{Capital}$$

The entity hypothesis also acquired the status of a theory.

The proprietorship and entity theories were attempts to explain accounting practices within the framework of a fairly simple model. However, while these hypotheses explained many accounting procedures, they were too simple to explain adequately all the practices of accountants. Accounting researchers who sought to explain accounting were forced to consider accounting practices in greater detail.

Careful observation of accounting practice revealed patterns of consistent behaviour. For example, it was observed that accountants tended to be pessimistic in measuring both revenues and expenses. Where judgement was necessary it was observed that accountants usually underestimated revenue and overestimated expenses. The result was a 'conservative' measure of profit. Similarly, it was observed that accountants behaved as if the value of the unit of account (money) remained constant. These observations of accounting practice led to the formulation of a number of hypotheses such as: 'that where judgement is needed, the conservative procedure is adopted' and 'that it is assumed that the value of money remains constant'. Hypotheses such as these were confirmed by many observations of accounting practice. As a result of their observations, many accounting researchers produced lists of confirmed hypotheses or theories about accounting practices which purported to explain what accountants did. In chronological order some of the more important explanatory theories were presented by Paton (1922) [5]; Sanders, Hatfield and Moore (1938) [6]; Gilman (1939) [7]; and Paton and Littleton (1940) [8]. By 1940, the effort to produce an explanatory theory of accounting was virtually exhausted.

Not all of the research effort during the period 1800–1955 was directed towards testing explanatory hypotheses. Two important works which criticised contemporary accounting and proposed new accounting systems were published. MacNeal attacked valuation and realisation procedures and Sweeney proposed adjusting conventional financial statements for changes in the general level of prices [9]. These books were the forerunners of the normative hypothesis period which began about 1955.

With some notable exceptions, therefore, the researchers of this period devoted their efforts to testing hypotheses which described, explained and justified the existing system. During this period the theoretical foundation for today's historical cost accounting system was established. Although the explanatory theories resulting from this

research increased the level of understanding of accounting practices, it also revealed procedures which many observers believed were unsatisfactory. The use of historical cost as an apparent measure of value, the conservative measurement of profit, the emphasis on objectivity, and the recording of money amounts as if the value of money remained constant were amongst the accounting practices widely criticised during the early fifties.

### 1955–1970

In 1955, R. J. Chambers published the first of a series of articles which were to exert a considerable influence on accounting research [10]. Chambers argued that accounting research should be much less concerned with justifying and explaining contemporary practice and much more concerned with the development of a better accounting system. Chambers' view received a good deal of support and for the next fifteen years academic research was largely directed towards the testing of normative hypotheses of accounting. Normative hypotheses are concerned with what 'ought to be' and prescribe the procedures that will achieve a given objective.

The period 1956–1970 saw a considerable amount of effort and achievement in accounting research. The search for a better accounting system resulted in four broad proposals for change.

The first, largely developed by Edwards and Bell, suggested that accounting should be based upon the current replacement cost of assets [11]. Edwards and Bell pointed out that contemporary accounting practices were deficient because they confused gains and losses from holding assets with gains and losses from selling or using assets. To overcome this deficiency, Edwards and Bell advocated matching current replacement cost instead of historical costs against revenue in order to distinguish between current operating profit and holding gains and losses.

The second, based upon the work of Sweeney, suggested that the historical cost financial statements should be supplemented with statements adjusted by an index of changes in the general level of prices [12]. In this proposal for change it was pointed out that contemporary accounting unreasonably assumed that the value of money remained constant. This meant that 'conventional accounting records at the present time suffer from this lack of comparability of the dollar at different points of time' [13]. In addition, there was no indication of the gain or loss in purchasing power from holding monetary items.

The third, based upon the work of Chambers, proposed that financial statements should be based upon the use of the current cash equivalents of assets [14]. Chambers argued that the objective of accounting should be to provide up to date information about an entity's ability to adapt to changes in its environment. If the environment in which the entity exists is changed in any way, the entity must adapt itself to the new environment or fail to survive. For a business, adaptation means the disposal of assets no longer appropriate and the acquisition of new assets more suited to the new environment. The ability of a firm to adapt is primarily dependent upon the cash which can be obtained by selling its assets. Chambers concludes that the balance sheet should show the current cash equivalents of the separate assets and that profit should be measured as the change in the firm's adaptive capital over the period.

The fourth, developed from the work of Bonbright [15], proposed that 'value to the owner' or 'deprival value' should be the basis of accounting procedures. The value to the owner of an asset is the amount which the owner should receive to compensate him for the loss of the asset. This legal approach to 'value' has been advocated by a number of writers including Baxter, Solomons and Parker and Harcourt [16]. In some cases the value to the owner of an asset will be the current replacement cost, in other cases it will be the net selling price and in a few cases it will be the present value of expected future cash flows.

Although the research effort aimed at finding a better system of accounting during this period was impressive, towards the end of the 1960s there was a marked change in the direction of accounting research. This change in emphasis resulted from dissatisfaction with the attempts to develop general normative theories of accounting [17] and a belief that accounting research methods should be more 'scientific' [18].

### 1970—the present

Beginning in about 1970 there was a return to the use of empiricism. Researchers once again turned their attention to the study of 'what is' rather than 'what ought to be' and the normative hypothesis period proved to be only a short interruption to the empiricism of accounting research. This 'neo-empiricism', however, is different from the empiricism of the earlier period. The empiricism of the earlier period was concerned with testing explanatory hypotheses, whilst neo-empiri-

cism is concerned with testing descriptive hypotheses and generally uses sophisticated statistical techniques. The hypotheses are primarily concerned with describing accounting and its environment and the researchers rarely offer explanations for the relationships that are hypothesised. Examples of empirical hypotheses that have been tested are: that changes in accounting methods are used to 'smooth' reported profits; that accounting data can be used to predict corporate failure; that profit based upon matching current revenue with current expenses is a better predictor of profits than conventional accounting profit and so on.

Ball has suggested that the first examples of neo-empiricism appeared in the late 1950s [19]. An index of published empirical research in accounting which he compiled shows that six empirical studies were published in 1956 and that sixteen empirical studies were published in 1970. A comparison of the contents of a 1976 issue of *The Accounting Review* with those of a 1966 issue will indicate the magnitude of the swing back to empiricism. In 1966 only a small proportion of the articles could be described as empirical but by 1976 virtually all articles were based upon empirical research. We have chosen 1970 as the beginning of the neo-empirical period because the testing of normative hypotheses had virtually ceased by that date, giving way to empiricism as the predominant research activity.

There are several possible reasons for this return to empiricism:

- (a) There was a desire to make accounting research more rigorous in order to improve the reliability of the results and to improve the level of understanding of accounting;
- (b) There may have been a desire to enhance the status of academic accountants in the scholarly community by assuming the mantle of a scientist. The use of 'scientific' methods suggests that the researchers are 'scientists';
- (c) An increasing number of accountants are able to use the sophisticated statistical methods necessary for empirical research. In addition, easily accessible data sources are becoming more readily available. Without a group of competent researchers and suitable data, neo-empiricism would have been impossible.

To summarise, we have suggested that accounting research has progressed through three stages. In the first stage, 1800–1955, researchers were primarily concerned with testing hypotheses which explained accounting. Towards the end of the

period, the insights into accounting which this research had provided led to a widespread dissatisfaction with accounting procedures. The dissatisfaction resulted in researchers testing hypotheses about improvements in accounting. The testing of normative hypotheses occupied researchers for about fifteen years between 1956 and 1970. Towards the end of this period there was increasing dissatisfaction with both the objectives and methodology of the researchers testing normative hypotheses. Accounting researchers largely turned to 'scientific' research and began to test descriptive hypotheses. This research is usually described as 'empirical'. Of the 178 years during which accounting research has been undertaken, 15 years were primarily devoted to testing normative hypotheses and the remaining 163 years were primarily devoted to testing hypotheses which described or explained accounting practice.

### **The effect of research on accounting practice**

#### *Testing of Explanatory and Descriptive Hypotheses*

In the previous section we suggested that the overwhelming majority of accounting research has been concerned with testing hypotheses which explain or describe accounting practice. The very nature of this research suggests that it is unlikely to have a dramatic impact on accounting practice. It is designed to improve our understanding of accounting rather than to change accounting. This does not mean, however, that this type of research has had no effect on accounting practice.

The testing of hypotheses which describe and explain accounting practice leads to a clearer understanding of what accountants do. This should result in a better text book exposition of accounting procedures and improve the teaching of accounting. The result should be a better trained professional accountant who not only knows what to do but why he does it. He should know when conventional practice is inappropriate and this could lead to better procedures. For example, in the USA the FASB required that as from 1 January 1976, firms must use the appropriate historical rates to translate amounts carried at past prices and the current rate for amounts carried at current or future prices, in order to prevent firms with foreign branches or subsidiaries employing inappropriate measurement practices when translating foreign accounts into US dollars [20]. Similarly, the increased understanding of accounting which results from testing explanatory and

descriptive hypotheses may reveal inconsistencies and undesirable procedures which could then be eliminated from practice. For example, in Australia, the Institute of Chartered Accountants required firms to depreciate their buildings as from 1 July 1971, in order to ensure that they comply more closely with the principle of matching [21].

Over time the cumulative effect of these improvements may be quite considerable. A comparison of an accounting text of 1900 with one of 1977 or published accounts of 1900 and those of 1977 will indicate that accounting has changed substantially. While not all of these changes can be attributed to the effect of research, there is no doubt that some of it is due to an increased understanding of the basic premises of accounting. However, it is frequently difficult to establish cause and effect. It may be argued that improved practice attracts the interest of researchers who then formulate and test hypotheses related to the improved practice. This is undoubtedly true in some cases. For example, the research into reporting to employees was stimulated by the fact that a number of firms prepared special financial reports for employees. A similar sequence is evident with other accounting practices such as purchase versus pooling and accounting for corporate social responsibility. Even in situations where changes in practice stimulate research it is probably unfair to suggest that the research has no effect on practice. First, research publicises the changes in practice and quickly brings them to the attention of practitioners. Second, research may suggest improvements or refinements in the procedures. Thus research facilitates the diffusion of the new techniques, possibly in an improved form.

It is concluded that whilst the testing of explanatory and descriptive hypotheses has not led to any dramatic impact on accounting practice the research has not been without effect. It has improved the understanding of accounting and this has resulted in better trained, more discriminating accountants. It may also have facilitated the diffusion of techniques and in some cases may have resulted in improvements in the new procedures.

#### *The Testing of Normative Hypotheses*

Many normative hypotheses were tested during the period 1956–1970. Each of the resultant theories proposed substantial revisions to the existing accounting system. These alternative accounting systems have been widely considered



and while each theory has its band of dedicated adherents, no particular proposal for change has yet won general approval. However, there have been indications, recently, that some of the alternatives are viewed favourably by the professional accounting associations in the USA, UK and Australia.

In the USA, a *Statement* was published in 1969 by the Accounting Principles Board (APB) recommending that financial statements restated for changes in the general level of prices should be presented in addition to conventional statements [22]. Companies were not required to publish these supplementary statements and it is evident that, by and large, companies have not followed the APB's recommendation. A survey of 600 companies in 1972 showed that none of the survey companies presented financial statements adjusted for changes in the general level of prices [23].

In December, 1974, the Financial Accounting Standards Board (FASB) issued an *Exposure Draft* in which it proposed that financial statements restated for changes in the general level of prices should be included in the annual report to shareholders in addition to the conventional statements [24]. It was proposed that the Standard should be implemented as from 1 January 1976. In November 1975, the FASB decided to defer further consideration of its proposal because the results of a field study showed that general purchasing power information was not sufficiently well understood by the preparers and users of financial statements to warrant the cost of implementing the Standard [25].

Despite this postponement, it is likely that the effect of price changes on firms will be recognized to a limited extent in the USA. A recent *Accounting Series Release* by the Securities and Exchange Commission (SEC) requires selected companies registered with it 'to disclose the estimated current replacement cost of inventories and productive capacity at the end of each fiscal year for which a balance sheet is required and the appropriate amount of cost of sales and depreciation based on replacement cost for the two most recent full fiscal years' [26]. It is interesting to note that whereas the FASB favours general price level adjustments, the SEC favours the recognition of specific price changes.

In the post-Wold War II period it has been the practice of an increasing number of companies in Australia and the United Kingdom to revalue their fixed assets periodically to reflect increases in prices. There are now moves to require

firms in those countries to incorporate the effects of changes in prices in their financial statements. In Australia there is a recommendation that as from 1 July 1978 current cost accounting as outlined in the *Statement of Provisional Accounting Standards DPS 1.1*, should be used for the preparation of financial statements [27]. Essentially, the Australian proposal calls for the current cost (in general this will be the asset's replacement cost) of inventory and depreciable assets to be reflected in the financial statements. Paragraph 1.03 of *DPS 1.1* summarises the operational features of current cost accounting as follows:

(a) the result of any one period of accounting is determined by matching the revenue for the period with the current cost of producing that revenue. To this end, the cost of goods sold is calculated (or adjusted) to reflect the current cost of goods when consumed. Similarly, depreciation charges are calculated (or adjusted) to reflect the current cost of the service potential of depreciable assets consumed or expired in the period. No adjustment is normally required in respect of any other costs brought to account as expenses for the period because such costs are already expressed in terms of the current prices of the goods or services to which they relate.

(b) In the balance sheet, the resources of the entity are stated, where applicable, on the basis of their current costs at balance date.

On the 30th November 1976, the Accounting Standards Committee in the United Kingdom released *Exposure Draft 18* on 'Current Cost Accounting' [28]. Essentially, the British proposal is that the profit and loss statement will show a charge against revenue for the replacement cost of inventory consumed and fixed assets used while the balance sheet will show the appropriate current values for most assets. The exposure draft also proposes the separate disclosure of the gains or losses from holding monetary items. Apart from this requirement with respect to monetary items, the current cost accounting systems proposed in the two countries are virtually the same.

It is apparent that while the research which produced the general normative theories of accounting has not yet caused a significant change in accounting practice, it appears to have influenced the attitude of accounting policy makers. They are now willing to consider major changes to accounting procedures. Sterling has

suggested that, in science, the chain of events giving rise to a change in practice is typically that research results lead to a change in practice through the education process [29]. Sterling argues that because accounting educators are largely preoccupied with teaching current accounting practice this chain is broken and research results as a consequence have little chance of influencing practice. In our opinion, Sterling overstates his case. The alternative accounting systems have been a part of accounting curricula, particularly in the UK and Australia, for many years. For at least the past decade entrants to the accounting profession have had a working knowledge of these alternative accounting systems. As the economist J. M. Keynes in a famous passage has written: 'Practical men, who believe themselves to be quite exempt from any intellectual influences are usually the slaves of some defunct economist. Madmen in authority, who hear voices in the air, are distilling their frenzy from some academic scribbler of a few years back' [30]. What is true for economists is equally true for accountants. The results of research on normative hypotheses are stored in the minds of accounting policy makers and it is possible as a consequence that the final link in Sterling's chain will be forged in the not too distant future.

It is concluded that normative theories have not, as yet, had any marked impact on accounting practice. However, there is some evidence that accounting policy makers are aware of the results of the research and there may yet be significant consequent changes in accounting practice.

Why has not the research which produced the normative theories of accounting had a greater impact on accounting practice? There are a number of possible reasons. First, it is possible that researchers proposing changes to accounting have failed to communicate clearly the ideas contained in their proposals for change. Mautz has suggested that 'if you want to reach me or people like me, use a language we can understand' [31]. If the ideas cannot be easily understood by accounting policy makers then it is not surprising that the ideas are not adopted in practice.

Second, because the process of setting accounting standards probably depends to a greater extent upon political rather than upon technical considerations, it is unreasonable to expect that 'good' theory and research will always become 'good' practice [32]. For example, Moonitz cites the influence of the US Congress in the determination of accounting principles: 'Back in the

1930s, Congress had to amend the tax law for LIFO to become acceptable. Much more recently, Congress intervened directly in the tax allocation problem by outlawing 'flow-through' accounting for public utilities. It also intervened directly in the investment credit situation by legislating choice on a taxpayer's part as to the accounting he wishes to follow' [33].

Third, there is, as yet, no agreement about the purposes of financial statements. For example, in the normative theory period there were four broad proposals for change, each of which is consistent with a different implied capital maintenance objective [34]. The acceptance of one of the capital maintenance objectives is simply a matter of opinion. Thus, if the accounting community could agree on the importance of maintaining intact a firm's adaptive capital then an accounting policy which involved measuring the current cash equivalents of firms' assets and liabilities would probably be implemented. It is not possible to agree on the form and content of financial statements without first having an agreed objective. The prospects for agreement on the form and content of financial statements depend upon achieving a consensus on the objective of financial statements [35].

## Conclusion

We set out to test the hypothesis that accounting research does not matter because it has had no effect on accounting practice. It is apparent that the majority of the accounting research since 1800 has been concerned with testing hypotheses that explain or describe accounting. The results of testing such hypotheses would not be expected to have a dramatic effect on accounting practice because the hypotheses are concerned with the existing situation. However, this research has had some impact on practice. It has helped accountants to understand more fully the procedures they adopt which, in turn, has led to a reduction in inconsistent practices. In addition, in some instances this research has led to improvements in procedures. In contrast, the accounting research that has resulted in normative theories of accounting has had virtually no impact on accounting practice, although there is every indication that it may yet have an impact on accounting practice.

Therefore, in answer to the question: Does accounting research matter? Our response is: Yes—but not very much.

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# The Report of the Meade Committee<sup>1</sup>

Simon James

Living as we do in an age of taxation, it is hardly surprising that enquiries into the operation of various tax systems and proposals for their reform now seem almost commonplace. At the top end of this new industry, however, there have been several impressive investigations in recent years. Two fairly well known studies were undertaken overseas, one by the Carter Commission<sup>2</sup> in Canada, and the other by the United States Treasury.<sup>3</sup> In the UK, the tax system has been subject to a fundamental examination by the Meade Committee, whose report appeared at the beginning of 1978.

The Committee was set up by the Institute for Fiscal Studies to examine the tax system as a whole, to establish the objectives of the tax system, and in the light of this examination to suggest possible reforms. During the course of its deliberations, the membership of the Committee underwent a number of changes but the final membership consisted of five economists, two accountants, one banker, one solicitor, one with connections with both law and accountancy, and an ex-Deputy Chairman of the Inland Revenue. A full list is given in the Appendix. The potential prominence of economics as a background to the study was reinforced by the Committee's chairman, Professor James Meade, who was awarded the Nobel Prize for Economics in 1977.

It was originally hoped that the Committee would complete its report in a year. The magnitude of such a task, however, soon became clear. The Committee, therefore, restricted itself to direct taxation and spent two years completing its report. In addition to direct taxation, the Committee examined social security contributions (which, of course, might legitimately be regarded

as a form of direct taxation), social security benefits and some aspects of value added tax.

Following an excellent analysis of the existing tax system, the Committee came to the conclusion that radical reforms would be necessary if the present anomalies were to be removed. As many readers may already know, the main recommendations included a shift to a progressive expenditure tax system, together with progressive taxation of wealth. The proposed tax system would also discriminate against inherited wealth, as opposed to wealth accumulated by an individual out of his own income. In addition, the Committee proposed the reform of the social security system by the introduction of what was described as a 'New Beveridge scheme'. Before discussing particular areas, it might be worth giving a fuller summary of the Committee's proposals.

With one exception, the members of the Committee were in favour of a move towards an expenditure rather than an income basis for taxation. It was thought that such a change would be feasible provided a system of self-assessment, such as that operated in the United States, were first introduced for direct taxation. A move to an expenditure tax would also have other implications, for example, regarding the migration of people and capital.

There would also be implications for corporate taxation, and several suggestions were made. One in particular was the movement of corporation tax towards a flow-of-funds basis. As might be guessed, one (but not the only) method of doing this would be to tax the amount by which a company's inflow of funds exceeded its outflow.

On capital taxation, two possibilities were put forward. One was an accessions tax levied on the receipt of gifts and inheritances. This would have the effect of exempting from capital taxation the wealth an individual had saved himself. However, if it were desired to tax both saved and inherited wealth, the Committee proposed a progressive annual wealth tax, together with a moderate accessions tax.

<sup>1</sup>*The Structure and Reform of Direct Taxation*. Report of a Committee chaired by J. E. Meade for the Institute for Fiscal Studies (London: Allen and Unwin, 1978, pp. xx + 533, £6.95).

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The proposals for the reform of social security contributions included *taxable* benefits to cover unemployment, old age and so on, but without special means tests. Other reforms in this area included raising tax thresholds to avoid the present overlap whereby a person can simultaneously be in receipt of social security benefit and subject to income tax. Further changes covered the reform of the treatment of married, as opposed to single, people and the introduction of a 'home responsibility payment' for those in charge of children or other dependents requiring home care.

In each of these areas, the Committee's Report provides a much more complex analysis and set of proposals than can possibly be captured in the above paragraphs. Nevertheless, it can be seen at once that the suggestions put forward are more radical than most proposed tax reforms.

### The structure of the Report

The Report is divided into four parts. Part One consists of three introductory chapters and Part Two of three chapters on the defects of the existing tax system. The book is dominated by the 16 chapters of Part Three which consider some radical reforms of the tax system. The final part of the book consists of a summary and conclusions.

One of the earliest chapters in the book is directed to the characteristics of a 'good tax structure'. One is reminded of the quip that 'the only good tax is one on somebody else'. Nevertheless, a discussion of the ideal tax system is clearly the place for a book on tax reform to begin. The problem in designing a good tax system, of course, is 'good' with respect to what? It would be relatively easy, for example, to design a tax system which promoted economic efficiency and incentives. It would also be relatively easy to design a system which effectively redistributed income and wealth in line with whatever society considers equitable. It is not so easy to design a system which is both efficient and fair.

The Committee quite rightly suggests that a good tax system is one that is coherent, simple and cheap to administer. On the other hand, if a tax system is to be considered equitable, then it usually has to be sufficiently sophisticated to take account of a wide range of different circumstances.

The merits of any particular tax system, therefore, largely depend on how far it achieves the

goals society considers desirable. All that one can do, as the Committee does, is to list those properties which are desirable, all other things being equal. One must then leave it to the political process to decide which sort of tax system is required in practice. The Committee, however, does make the excellent point that a tax structure should command a wide political consensus and should allow for a wide range of possible tax policies without the need for radical changes. If a new government can implement its policies by changing the rates of tax rather than the taxes themselves, then the tax system scores well on grounds of stability.

Part Two of the Meade Report is concerned with a description of the existing tax system together with an account of its defects. Now, as everyone knows, finding defects and anomalies in the British tax system is a sport which almost anyone can play. And most people do. Nevertheless, the Committee provides an impressive catalogue of the maladies of the tax system.

There are several points which might be mentioned. One is the way that income tax, capital gains tax and corporation tax interact to produce a curious range of rates of return on similar assets for similar people. For instance, the Report considers the case of equity finance for incorporated companies (pp. 63 to 66). As an example, it supposed that a company invests in an asset yielding 10% per annum after allowing for depreciation. Under a number of reasonable assumptions, it appears that the final net-of-tax return to shareholders varies widely, and it does so even for taxpayers who face the same marginal tax rate. In the Report's own words 'for a taxpayer with a zero marginal rate the yield varies from 14.9% to 4.8% and for a taxpayer with a 98% marginal rate from 7% to 0.1% (p. 65). When other forms of finance and private businesses are considered, the range of possible yields increases further.

The reason for what seems to be a totally arbitrary range of yields lies in a number of factors. They include considerations such as whether the business is incorporated or not, whether the finance takes the form of equity capital or a loan and whether the return on the investment is distributed or retained by the business to be re-invested. The Committee's report makes a splendid job of showing the oddities of the tax treatment of capital markets. And it is the reform of this area that is one of the Committee's main aims.

Another anomaly which might be raised in passing concerns the treatment of married

women. The combined effect of the married man's allowance and the wife's earned income allowance is that, where the husband is the only breadwinner, the couple would be entitled (in 1977-78) to an allowance of £1,455. However, if the wife were the only breadwinner they would be entitled to £2,400: strange indeed!

Many other aspects of the present tax system are considered in detail in the Report. One is the interaction between income tax, national insurance contributions and social benefits which leads to the problems of the poverty trap and so on. There is also an entire chapter devoted to the problem of inflation. The bulk of the Report, however, is concerned with the reform of the tax system rather than its current operation. Part Three begins with a chapter on income tax and three chapters on expenditure taxes.

### **Income and expenditure taxes**

The Committee spent some time discussing the implications of introducing a comprehensive income tax (CIT) in the UK. The tax base for a CIT might be thought of as the 'accretion of purchasing power', which is perhaps a concept more familiar in the USA than in the UK. A CIT would include not only 'income' as it is widely understood, but also capital gains and other capital receipts. In practice, an individual's comprehensive income could be measured by adding his consumption to changes in the net value of his assets over a period.

As the Committee points out, the existing income tax is far from being a comprehensive income tax in the sense described, and in some areas is even similar to an expenditure tax. For example, two of the main methods of saving in the UK—contributions to approved pension schemes and life assurance premiums—are given favourable treatment under the present income tax. On the other hand, as we have just seen, the interaction of income tax, capital gains tax and corporation tax can, in some circumstances, result in heavy taxation of savings.

A comprehensive income tax base would have several advantages. In particular, it would reduce the incentives for taxpayers to manipulate their affairs for the sole reason of avoiding tax. One of the best examples at present, is the extent to which it is possible to convert income (narrowly defined), which is taxable at rates up to 98%, into 'capital gains', which are subject to a maximum rate of 30%. In its pure form, a CIT would also

largely do away with the need for the separate taxation of capital transfers.

Nevertheless, there are several problems with a CIT. In practice, it is exceedingly difficult to measure a person's 'comprehensive income'. In addition, it would involve very serious administrative complexity with regard to adjustments for inflation. Although both progressive income and progressive expenditure taxes require adjustments to the thresholds for tax and higher rates of tax, comprehensive income tax requires additional adjustments to nominal capital gains and losses. This immediately raises a whole range of problems. For example, should capital losses be allowed for the decline in the value of money? Adjustments would also have to be made where interest payments are deductible for tax purposes. This would mean that when inflation exceeds the rate of interest, as has often been the case in the recent past, we would observe negative mortgage relief. It does not require much imagination to see the political implications of that. The Committee stresses that these inflation-adjustment problems would not arise with an expenditure tax as much as with a comprehensive income tax. For these reasons, as well as more direct arguments in favour of an expenditure tax, the Committee came down firmly in favour of a tax based on expenditure.

The essential idea behind an expenditure tax is that it is levied only on consumption expenditure. Income that is saved or invested is not taxed until the savings or investment are realised and consumed. Saving is therefore treated more favourably by an expenditure tax than by an income tax. With the latter, savings are taxed once when the amount saved was originally received as income, and taxed again when the saving yields interest or profit. An expenditure tax taxes savings only when they are spent. It therefore allows a higher rate of return to saving because interest can be received on the gross amount of any income saved.

The Committee presented four possible ways by which the principle of an expenditure tax could be operated in practice. The one which found most favour with the Committee would require a taxpayer's consumption expenditure to be calculated in each period as follows:

- (1) The taxpayer's total realised income would be established.
- (2) Any capital receipts, such as the proceeds from the sale of capital assets and sums borrowed, would be added on.

(3) All expenditure for purposes other than consumption would be deducted. This would include expenditure on capital assets, and sums lent out or repaid.

(4) The final figure would be the amount spent on consumption.

The main advantage of this method over others is that, by providing a figure for each taxpayer's total consumption, it would permit an expenditure tax to be levied at a series of progressive rates.

The second method of taxing spending rather than saving would be a tax on value added. The third would be an income tax which exempted investment expenditure with a system of 100% capital allowances, and the final method, again an income tax, would exempt investment income from tax.

After considering these various possibilities, the Committee came down firmly behind two particular forms of expenditure tax. The first was a 'universal expenditure tax' (UET) which would be based entirely on the first method described above. The second was a two-tier expenditure tax (TTET). The lower tier would consist of a single basic rate levied through a value added tax. The upper tier would be a restricted version of the UET and would allow tax to be levied at higher rates on taxpayers with higher levels of expenditure. It would work in a similar way to the pre-1973 income tax system whereby most taxpayers were subject to tax at the standard rate, and more prosperous individuals to surtax as well. A TTET could be introduced more gradually than a UET, and could also provide a transitional stage in a move to a fully-fledged UET.

The idea of an expenditure tax is not a novelty. Its history can be traced back to Thomas Hobbes and it has been discussed by people such as John Stuart Mill, Alfred Marshall, Irving Fisher and Lord Kaldor. In fact, Kaldor<sup>4</sup> deals with the arguments for an expenditure tax more fully than does the Meade Report. The implications of an expenditure tax have also been discussed recently overseas. Professor Sven-Olof Lodin designed an expenditure tax for the Swedish Royal Commission on Taxation and *Blueprints for Basic Tax Reform*<sup>5</sup> discussed the tax in the context of the United States.

The Meade Committee's arguments in favour of an expenditure tax might be summarised as

follows. First of all, it is suggested that 'a progressive expenditure tax gives the maximum opportunity for business enterprise and development since all resources devoted to such development are free of tax' (p. 502). The Committee presents a sharp example of this: 'With a progressive income tax, a wealthy man with a high marginal rate of income tax of 83% will be able to use only £17 out of £100 of profit for the development of his business, whereas with a progressive tax on expenditure he could use all his profit to develop his business' (p. 33). Essentially the argument is that by exempting saving from tax, the level of saving would rise. As a result, investment would rise and the economy would expand.

A second argument is that a progressive expenditure tax would levy 'a heavy charge on those who live at a high standard of consumption whether it is based on a high income or on the dissipation of capital wealth' (p. 6). Also, 'A strong case can be made for this expenditure base in that it levies a tax on the claims which a taxpayer makes at any one time on the community's resources which he uses up for his own consumption purposes. If he saves his income instead of consuming it, he is putting resources back into the productive pool' (p. 33).

A further argument is that a radical change to a new system would simplify the direct tax in all sorts of ways. It is suggested that the only way to achieve major simplification is to start again with a new system.

There are, however, several limitations to the arguments in favour of an expenditure tax. First of all, there does not appear to be a strong relationship between the level of saving and the return to saving. It is not even clear that economists fully understand what does determine the level of savings. Before the first half of the 1970s, many economists argued that the level of savings was reasonably stable. However, during the early 1970s the ratio of personal savings rose from its normal level of around 8% to over 12% of personal disposable income. The same phenomenon occurred in other Western countries, and did so following a period when the real rates of return to many forms of saving were mainly negative. It is not clear, therefore, whether the introduction of an expenditure tax would increase saving simply because it increased the rate of return to saving.

Even if it did increase saving, it is not clear that this would increase economic growth. Keynes was not the first economist, and doubtless will not be the last, to suggest that saving carried

<sup>4</sup>Nicholas Kaldor, *An Expenditure Tax* (London: Allen and Unwin, 1956).

<sup>5</sup>*Op. cit.*



to excess can lead to a fall in aggregate demand and economic recession.

A third counter argument is that work incentives might be more adversely affected by an expenditure tax than by an income tax. Because savings are treated relatively favourably by an expenditure tax, it is likely that the tax would have to be levied at higher rates than an equivalent income tax. It is not certain *a priori* whether people would work harder or less hard when faced with higher rates of tax.<sup>6</sup> Nevertheless, it is possible that if people work mainly in order to consume now rather than to save for the future, then an expenditure tax might discourage work effort. If it did, and incomes fell, then the absolute level of savings might also fall, even if the proportion of income saved rose.

A fourth point is that if one takes a taxpayer's life-cycle as a whole, then hardship is more likely to be imposed in certain periods under an expenditure tax. The tax would impose a greater burden in the years when a family's spending is high relative to its income. The two most important examples that come to mind are families setting up home and with children, and pensioners living off their savings during retirement.

Apart from these rather immediate considerations, the expenditure tax proposals can be discussed from a different angle. One can easily agree with any proposals for a total clean up of the existing defects and anomalies in the tax system. One can also be sympathetic with the proposition that it is preferable to introduce a new and coordinated system rather than continue to patch up the existing tax structure in an ad hoc manner. However, in a modern democracy there are always powerful forces ready to press for their particular causes. Everyone is in favour of simplicity in general, but complexity in particular cases. How long would it be before a new streamlined expenditure tax became as complex and uncoordinated as our present vintage income tax? Presumably the pressure would then be to introduce a simple income tax!

Some idea of the potential complexity of an expenditure tax in practice can be gained from looking at attempts to introduce such a tax in the past. Plans for a personal and progressive 'spendings' tax were submitted by the United States Treasury to the Senate Finance Committee in 1942. The proposed tax had the twin aims of controlling inflation by taxing private spending

and providing funds for war finance. The tax was to have been administered within the existing arrangements for income tax, but nevertheless was rejected by Congress largely because of the novelty and complexity of the proposals. In what seems to have been a typical reaction at the time, Senator Harry Byrd described the proposed tax as 'the most complicated and unworkable plan' the US Treasury had submitted in nine years.<sup>7</sup>

In India, following a report to the Indian Government by Kaldor,<sup>8</sup> an expenditure tax was introduced in 1958. Although the Indian Government had accepted Kaldor's proposals in principle, the tax as enacted differed substantially from the original proposals. Certainly it could not be described as a simple tax, and its repeal in 1962 was apparently 'welcomed in all quarters'.<sup>9</sup> However, 'the joy was short-lived' when the tax was reintroduced in 1964. The revived expenditure tax also proved to be short-lived, and was repealed once more in 1966. Although the Finance Minister at the time felt that 'on purely economic grounds, it would be a very sound principle to replace the income tax increasingly with a tax on expenditure', he went on to say 'Given... the administrative difficulties and inconvenience to assesseees involved in the assessment of expenditure, it is, however, not possible to attempt this substitution on any significant scale at the present stage'.<sup>10</sup>

In Ceylon (now Sri Lanka) an expenditure tax was introduced in 1959, also following proposals made by Kaldor to the Government. The tax was repealed in 1963, though it has been reintroduced on a limited basis in 1976. Taking these three experiences together, therefore, it can be seen that previous attempts to introduce an expenditure tax have not been showered with success.

One other point might be made regarding earlier proposals for expenditure taxes. In the US it was proposed that the expenditure tax be imposed *in addition to* the income tax, and in India and Sri Lanka income and expenditure taxes were operated side by side. It must be a political possibility, however remote, that if an expenditure tax were to be introduced in the UK it would be levied not instead of but on top of the existing

<sup>7</sup>Quoted in Randolph E. Paul, *Taxation in the United States* (Boston: Little, Brown 1954), p. 312.

<sup>8</sup>N. Kaldor, *Indian Tax Reform* (New Delhi: Ministry of Finance, Government of India).

<sup>9</sup>K. C. Khanna, 'An Expenditure Tax in India', *Bulletin for International Fiscal Documentation*, 1964, p. 361.

<sup>10</sup>Quoted in the *Bulletin for International Fiscal Documentation*, 1966, p. 201.

<sup>6</sup>*Cf.* Simon James and Christopher Nobes, *The Economics of Taxation* (Oxford: Philip Allan, 1978), pp. 46-63.

income tax; and in some ways this possibility is not far removed from the Meade Committee's proposals for a two tier expenditure tax. Nevertheless, despite the implications for the UK of the experiences of the US, India and Sri Lanka, the Meade Report does not discuss this historical aspect of expenditure taxation.

The introduction of an expenditure tax in the United Kingdom would also raise many practical administrative problems. First of all, there would be the transitional problems associated with any major change of the tax system—especially in relation to the re-education of taxpayers, tax advisers and tax administrators. Moreover, with a switch from an income to an expenditure tax base existing savings require special consideration. When savings (such as pensions) have been accumulated under an income tax regime, it is hardly fair to penalise them when they come to be spent under an expenditure tax regime. Like many other similar problems, it could be overcome, but only at the cost of greater administrative complexity and expense. A similar problem arises with emigration. Unless there were special provisions, individuals would be able to save under a favourable expenditure tax in the United Kingdom, and then emigrate to a country with an income tax system to dissave. One possible answer might be some sophisticated form of emigration tax but it is hard to see any satisfactory solution to this problem.

Another area to be considered is self-assessment. The Committee concluded that a universal expenditure tax could not be introduced without self-assessment—probably along the lines of the system operated in the United States. Yet this, in itself, is a major reform.<sup>11</sup> The Inland Revenue's position<sup>12</sup> is that self-assessment is not a practical proposition until the computerisation of PAYE is completed, which is unlikely to be before the mid-1980s. The introduction of a fully fledged personal expenditure tax may not therefore be possible until the 1990s.

To summarise, there appears to be a number of reservations about the wisdom of switching to an expenditure base for taxation, both in principle and from an administrative point of view. Some serious thinking would need to be done before the personal tax system is subject to what may

very well turn out to be its largest upheaval since the introduction of income tax in 1799.

### The taxation of capital

The Report justifies the taxation of wealth on familiar grounds. These include the argument that wealth confers benefits on its owners over and above any pecuniary yield and that society may wish to use capital taxation to encourage a more equal distribution of wealth. It is also argued that inherited wealth should be taxed more heavily than wealth which has been accumulated out of an individual's own income. The Report divides the taxation of wealth into two areas. The first is the taxation of wealth as it is transferred between individuals. This, of course, is the basic method of capital taxation in the United Kingdom. The second area is the taxation of capital itself through some form of wealth tax such as those employed in a substantial number of countries overseas.

In principle, the taxation of capital transfers could be incorporated into an income or expenditure tax system. Gifts and bequests could be treated as the donee's income. Conversely they could be considered as part of the donor's taxable expenditure. The major problem with such an arrangement is that a complex system of averaging would be required, otherwise large capital transfers might push some taxpayers into income or expenditure tax brackets that were inappropriately high. The Committee also felt that the tax system should be specifically designed to encourage a wide distribution of inherited wealth.

As a result of these and other considerations, the Committee came out strongly in favour of an accessions tax. As its name suggests, an accessions tax would be based on the cumulative amount received by the *donee*, unlike the present capital transfer tax which is levied on the cumulative amount given away by the *donor*. One of the main implications of this can be seen by imagining a rich individual who is considering which of his relatives he should leave his wealth to. With capital transfer tax, the final liability is not affected by his decision. With an accessions tax, however, liability would be inversely related to the value of the gifts and bequests already received by the relatives.

There would be a potential drawback if the accessions tax failed to take account of the length of time the inherited wealth were held. This would, for example, mean that an inheritance held for a year would be subject to the same amount

<sup>11</sup>See N. A. Barr, S. R. James and A. R. Prest, *Self-Assessment for Income Tax* (London: Heinemann Educational Books, 1977).

<sup>12</sup>Board of Inland Revenue, *120th Report* for year ended 31 March 1977 (HMSO Cmd. 7092, January 1978), Ch. V.

of tax as a bequest held for fifty years. There would also be an incentive for generation-skipping, e.g. leaving wealth to grandchildren rather than children. To some extent, a straightforward annual net wealth tax would reduce the problem but the Committee felt it necessary to suggest particular forms of tax which were specifically designed to overcome the difficulty.

With an unusual preference for acronyms, the Committee came up with three possibilities, namely: AGAWAT, PAWAT and LAWAT. AGAWAT (or an age-gap-annual-wealth-accessions tax) did not find a great deal of favour with the Committee and so will not be discussed here. A more likely possibility was PAWAT (a progressive-annual-wealth-accessions-tax). As with many other types of accessions tax, PAWAT would require the maintenance of a cumulative record of each taxpayer's capital receipts. However, the rates of PAWAT would also be related to the length of time a person enjoyed an inheritance. This would be done by taking account of the age of the taxpayer, so that the younger the taxpayer the higher the rate of tax. In addition, when the donor came in his turn to pass on the inheritance, the process would be reversed. A tax refund would then be made for those years the donor did not, in fact, possess the inheritance. The tax would therefore apply only to the years in which the inheritance was kept. Unlike a general wealth tax, PAWAT would apply only to inherited wealth.

PAWAT has the advantages of directly relating the level of tax to the period of ownership and also of greatly reducing the incentive for generation skipping. However, as the Committee acknowledged, there would be significant administrative problems. One example is that changes in the rates of tax would complicate the provisions concerning repayments when inherited wealth was passed on. Another is that the treatment of transfers between married couples would raise particular problems for PAWAT.

A similar version of PAWAT was LAWAT (linear-annual-wealth-accessions tax). The essential difference between the two is that LAWAT would be levied at a single rate, though it could be operated together with a progressive tax on all forms of wealth.

Whether a PAWAT or LAWAT stand any chance of being introduced within the foreseeable future is another matter. The taxation of transfers of wealth was reformed as recently as 1975 when capital transfer tax (CTT) replaced estate duty. It is very doubtful whether this area could or

should be subject to further major reform for some time. Furthermore, one of the experiences from the introduction of CTT (and for that matter capital gains tax) was that the potential complexities of a tax tend to multiply furiously when a tax is actually introduced. Potentially, PAWAT is certainly a complicated tax. And it is not clear that the non-progressive feature of the simpler LAWAT would find a great deal of Parliamentary support, even if it were accompanied by a progressive wealth tax. Nevertheless, these proposals skilfully combine the principle of an accessions tax with many ingenious safeguards against problems such as generation-skipping. All in all, they form an interesting and original approach to transfer taxation.

Less surprising are the Report's proposals regarding a tax on wealth itself. This is largely because the ground has been fairly heavily worked over in recent years, both by three members of the Committee itself,<sup>13</sup> and by the Select Committee on a Wealth Tax.<sup>14</sup> It is perhaps sufficient to say that after some discussion of the various issues relating to a possible wealth tax, the Committee concluded in favour of *either* a PAWAT with a low threshold and highly progressive rates, *or* a LAWAT combined with a progressive annual wealth tax.

It might be noted, however, that while the Committee's proposals on capital taxation are consistent in themselves, they do appear to sit a little uneasily with the proposals for an expenditure tax. Basically the Committee was in favour of the taxation of wealth on grounds of equity, while it argued for an expenditure tax largely on grounds of economic efficiency and incentives, notably on the incentives to save and invest. A wealth tax might well reduce economic inequality since it would tax the rich more than the poor. Yet an expenditure tax would work in the opposite direction in so far as the rich save more than the poor. Secondly, in as much as wealth is accumulated saving, a wealth tax might discourage the very saving an expenditure tax is supposed to promote.

### Corporation tax

The Committee favoured the retention of some form of corporate taxation. The case for such tax-

<sup>13</sup>C. T. Sandford, J. R. M. Willis and D. J. Ironside, *An Annual Wealth Tax* (London: Heinemann Educational Books, 1975).

<sup>14</sup>Select Committee on a Wealth Tax (HMSO, H.C. 696 I-IV, 1975).

ation is not, of course, unarguable since the profits of a business are ultimately taxable in the hands of the owners.

Four arguments were advanced (p. 227) as justifying a tax on incorporated enterprises. The first was that incorporation confers certain benefits, especially limited liability, which might justify an additional levy. The second was that owners of unincorporated businesses suffer personal income tax on their profits each year and it would be inequitable not to have some form of annual tax on incorporated companies' profits as well. The next argument suggested that the government has to raise a certain amount of tax and companies might be as good a target as any other. The final justification revolves around the old adage that 'an old tax is a good tax'. The point is that people adapt their affairs to take account of taxation, including corporation tax. Therefore, if the tax were suddenly removed, shareholders would receive unexpected capital gains.

None of these arguments is totally convincing. However, if it is desired, as the Meade Committee desires, to retain some form of corporation tax, the question becomes what sort of tax it should be.

The Committee concentrated on two main types of corporation tax, one with a profits base, the other with a flow-of-funds base. As the Committee points out, the existing UK corporation tax has moved a considerable way from being a tax on pure profits. The two main reasons are the lack of adequate adjustment for inflation and the existence of a wide range of 100% capital allowances.

Even if corporation tax could be moved on to a pure profits base, the Committee still preferred a flow-of-funds base. The thinking is basically similar to much of the thinking behind an expenditure tax and centres around the rate of return on re-invested profits. With a profits-based corporation tax, such investment could only be made out of profit which had already been taxed. And when the investment in its turn yielded profit, that would be taxed as well. In contrast, a flow-of-funds tax would not be levied on profits that were re-invested. Tax would only become payable when the investment yielded profits which were spent, or when the original investment were realised. The rate of return on re-invested profits is therefore lower under a profits-based tax than it would be under a flow-of-funds corporation tax.

After considerable discussion, the Committee came down in favour of a tax based on the excess of the inflow over the outflow of funds on all

transactions (apart from those concerning shares in UK companies).

There are, however, several administrative problems which would have to be considered before such a tax could be introduced. One is how the tax could be fitted into the EEC proposals relating the harmonisation of imputation systems. Another is that the proposed system raises several important issues regarding double taxation agreements.

### Social security and income maintenance

As mentioned above, the Report ably describes the problems arising from the interaction of national insurance, social security benefits and income tax. The system appears to be needlessly complex: for example, there is no consistent principle determining which social security benefits are taxable and which are not. One particular area of concern is that the combination of a low tax threshold and the withdrawal of income-related benefits (and there are about 40 of these)<sup>15</sup> can mean that individuals can face implicit tax rates in excess of 100%. It is also pointed out that, despite the complexity of the arrangements, the provision for many individuals is less than adequate. This is partly a result of inadequate benefits and partly because people do not always claim the benefits to which they are entitled. On top of this, it is clear that the system is both difficult to understand and expensive to administer.

Reforming this area provides a particularly difficult challenge. After discussing four possible schemes for reform, the Committee decided in favour of a return to the principles outlined in the Beveridge Report.<sup>16</sup> The main features of the 'New Beveridge' scheme may be summarised as follows:

- (1) All social insurance benefits would be increased to a minimum standard of living, namely to the supplementary benefit level. There would also be a 'home responsibility payment' where children or other dependents needed home care.
- (2) The tax and social benefit systems would be co-ordinated. In particular, the threshold for income tax would be raised to the supplementary benefit level.

<sup>15</sup>In evidence submitted to the Select Committee on Tax-Credit (Volume 1, HMSO 341-1, 1973) the DHSS and Inland Revenue stated that there were 42 means-tested benefits in England and Wales and two more in Scotland.

<sup>16</sup>Sir William Beveridge, *Social Insurance and Allied Services* (HMSO, Cmd. 6404, November 1942).

(3) Tax allowances for children would be replaced by cash payments (which is already happening) but the payments would be increased to the supplementary benefit level.

These proposals represent a bold attempt at serious social reform. Yet one cannot refrain from mentioning one or two reservations. The first is that a major hurdle to the implementation of the changes would be the cost. According to the Committee, the revenue required (in 1976/77) would be between £2,000 million and £3,600 million. If this sum were to be raised through income tax, it would imply an increase in the basic rate from 33% to between 37% and 40% which may well prove to be politically unacceptable. Secondly, the principle of flat rate benefits may no longer find a great deal of political support. Individuals have become accustomed to paying earnings-related contributions and might very well balk at losing rights to related benefits. Indeed, the earnings-related principle is a basic feature of the new state pension scheme introduced in April 1978. Rather surprisingly, this scheme is hardly mentioned by the Committee.

On the revenue side, the Committee suggests the integration of national insurance contributions and investment income surcharge into the income (or possibly expenditure) tax system. There is certainly a strong case for regarding national insurance contributions as a tax since most classes of contributions are compulsory and are not always closely related to entitlement to national insurance benefits. For example, Class 4 contributions do not attract any benefit at all. Nevertheless, it may well be the case that taxpayers think there is a close relationship between contributions and benefits. It might therefore be easier to extract revenue under the title of 'contribution' rather than with the title 'income tax'. Such thinking might have been responsible for the description of the new payroll tax, introduced in 1976, as a 'National Insurance Surcharge' – even though this tax is paid into general revenue rather than the National Insurance Fund.

## Conclusions

There is no doubt that the efforts behind the Meade Report were well worthwhile. Given the complexity of the modern British tax system, it

must be exceedingly difficult to produce a coherent package of reforms with the breadth and depth of the Committee's proposals. In addition, the Report contains many penetrating insights into the operation of the present tax system, and the entire report is written with a clarity that is not always to be found in discussions of such complicated issues. The Report will certainly remain an authoritative volume for many years.

Whether the proposed reforms are likely to be implemented, however, is perhaps another question. It is quite conceivable that the Committee's proposals will fail to find sufficient political support. As Professor Meade himself says in a personal foreword to the Report, one possible political reaction might be for the 'left' to reject it on the grounds that it gives incentives for 'capitalist enterprise' to invest, and for the 'right' to reject it because of the proposals relating to inherited wealth. There are other reasons as well why politicians may hesitate to accept the Meade package.

The implementation of the package would also present many practical difficulties. The Committee has not tried to dodge or minimise these problems and has made every effort to deal with them. Indeed, many of their solutions are highly ingenious. Nevertheless, with limited resources the Committee could hardly be expected to find satisfactory answers to all the administrative issues raised by the proposals. This, however, should not be used as the main measure of the Report's success.

The real achievement of the Committee has been the production of a comprehensive and thought-provoking review of the direct tax system as a whole. The Committee has also provided a timely reminder to those who would tinker with the tax system, that the coherence of the overall tax structure is at least as important as particular provisions and special cases. As William E. Simon, Secretary of the US Treasury, said in an American context, the nation should 'have a tax system which looks like someone designed it on purpose'.<sup>17</sup>

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**Acknowledgement** I am grateful to Professor David Walker for helpful comments on an earlier draft of this article.

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<sup>17</sup>*Blueprints for Basic Tax Reform*, op. cit., p. 1.

## Appendix

### The final membership of the Committee

Professor J. E. Meade, CB, FBA  
(Chairman)  
D. J. Ironside (Deputy Chairman)

J. F. Avery Jones

L. R. Bell

J. S. Flemming

J. A. Kay  
M. A. King

M. G. Macdonald

Professor C. T. Sandford  
Professor G. Whittington

J. R. M. Willis, CB, CMG

Emeritus Professor of Political Economy,  
University of Cambridge  
Partner, Thomson McLintock & Co., Chartered  
Accountants; Visiting Fellow, University of Bath  
Partner, Speechly Bircham, London, Solicitors;  
Joint Editor, *The British Tax Review*  
Retired partner, Thomson McLintock & Co.,  
Chartered Accountants  
Fellow in Economics, Nuffield College, Oxford;  
Managing Editor, *The Economic Journal*  
Fellow in Economics, St John's College, Oxford  
Now Esmée Fairbairn Professor of Investment,  
University of Birmingham  
Now Senior Lecturer in Accounting, University of  
Kent  
Professor of Political Economy, University of Bath  
Professor of Accounting and Finance, University  
of Bristol  
Retired Deputy Chairman, Board of Inland  
Revenue; Visiting Professor, University of Bath

### Walter Taplin Prize

The Association of University Teachers of Accounting, the Council of Departments of Accounting Studies and *Accounting and Business Research* offer a prize of £75 for the best article published in each annual volume. The prize is named in honour of the journal's founding editor, Walter Taplin.

The winning article is chosen by the subscribers. Their choice in 1976/77 was Edward Stamp, *ED 18 and Current Cost Accounting: A Review Article* (Spring 1977).

### Forthcoming Conferences

The 1979 Conference of the Association of University Teachers of Accounting will be organised by Middlesex Polytechnic and held in London on April 9, 10 and 11.

The Third International Congress of Accounting Historians will be held at the London Business School on August 16, 17 and 18, 1980 during the centenary year of the Institute of Chartered Accountants in England and Wales. A special Accounting History issue of *Accounting and Business Research* will be published in 1980.

# Income-Group Inflation Rates and General Purchasing Power Adjustments: an Empirical Test of the Heterogeneity Hypothesis

K. V. Peasnell and L. C. L. Skerratt

This paper reports some empirical results, based on UK data, concerning the extent to which a total consumer index can reflect the changes in the purchasing power of groups of individuals. For one criticism which has been made of restatement of historic cost or current value accounts—or indeed the summaries of past results<sup>1</sup>—in terms of units of constant purchasing power is that the rate of inflation varies for different individuals and entities, and hence adjustment by one general index cannot be helpful to all users of financial statements. In principle, this heterogeneity hypothesis is testable.

The report is divided into five sections. Section one discusses the rationale behind the hypothesis given in the accounting literature. Section two reports the approach adopted in this study to measuring changes in consumer prices. Section three outlines the main features of the research design, and the main empirical results are presented in section four. These results are evaluated in the final section of the paper.

## The hypothesis

Sterling has given the following rationale for the heterogeneity hypothesis that the rate of inflation varies significantly between individuals:

A perfect price index is a purely personal concept and is applicable only to that person. ... Obviously, the meaningfulness of any index varies inversely with the degree of deviation from the assumed pattern of purchases. The pattern is unlikely to fit any particular person perfectly, and it may vary widely. The consumer price index includes meat and milk

which have little relevance to a playboy who consumes wine, women, and song.<sup>2,3</sup>

Gynther and Sandilands present similar arguments.<sup>4</sup>

It is essential to distinguish this hypothesis, which is the object of enquiry here, from other related hypotheses. One of these concerns the issue of whether the restated money unit can be viewed as having constant *absolute* purchasing power. Gynther, in criticising Moonitz's<sup>5</sup> rationalisation of the constant purchasing power argument, has pointed out that:

The power to buy those goods varies from city to city in the USA just as the purchasing power of a sum of money will vary from city to city in almost any country. (In some places, prices of identical commodities even vary significantly between working class suburbs and top executive suburbs in one city)....

The 'purchasing power' of the dollar *does* vary from place to place, and significantly so in some cases. It is *not* 'generalized'. The purchasing power of the dollar is even different for two people living side by side and for two companies trading side by side in New York City. They have different spending patterns.<sup>6</sup>

In support of his argument Gynther presents

<sup>2</sup>R. R. Sterling, *Theory of the Measurement of Enterprise Income*, University Press of Kansas, 1970, p. 339.

<sup>3</sup>We are not inferring of course that Sterling fully accepts the truth of the hypothesis and the implication that the hypothesis (if true) invalidates the making of CPP adjustments; indeed he argues to the contrary: '...subsequent reports should adjust the previous reports by the Consumers Price Index'. *Ibid.*, p. 361.

<sup>4</sup>R. S. Gynther, *Accounting for Price-Level Changes—Theory and Procedures*, Pergamon Press, 1966; Sandilands Committee, *Report of the Inflation Accounting Committee*, Cmnd 6225, HMSO, 1975, ch. 2.

<sup>5</sup>M. Moonitz, 'Price Level Accounting and Scales of Measurement', *Accounting Review*, July 1970, pp. 465–475.

<sup>6</sup>R. S. Gynther, 'Why Use General Purchasing Power?', *Accounting and Business Research*, Spring 1974, pp. 141–157.

<sup>1</sup>As recommended in the Accounting Standards Committee discussion paper, *The Corporate Report*, 1975.

USA Labor Department statistics of how much it costs an urban family to live in different cities, for different standards of living. He shows for example that, on the lowest of three budgets, in 1967 it cost approximately 17% more to live in San Francisco than it did in Atlanta.

However, interesting though this absolute purchasing power hypothesis is, it is distinct from the heterogeneity hypothesis and, more important, it is irrelevant to the adjustment of accounting data for changes in the general purchasing power of money. The purchasing power of the dollar at a point in time could be lower in one city than another, or be different between individuals in a given city, even in the absence of general price inflation or indeed when *all* prices remain constant through time. The important point to note is that there is a distinction between prices of goods at a point in time and their rates of change over time; the correlates of these are the cost of (a particular standard of) living, on the one hand, and rates of change in the cost of living, on the other. General-index adjustments in accounting attempt to reflect *changes* in the purchasing power of the dollar; hence, the important question relates to whether the changes in individuals' costs of living are heterogeneous and not to whether they have different costs of living.

It also is important to distinguish the heterogeneity hypothesis from the hypothesis that a general consumer index such as the Retail Price Index '... is unlikely to represent the rate of inflation experienced by companies the content and pattern of whose spending is not reflected even approximately in the calculation of the index'.<sup>7</sup> If this hypothesis were true, it would be regarded by some (e.g. Sandilands and Gynther) as sufficient grounds for not adjusting accounting data by a general consumer index, and evidence concerning the heterogeneity hypothesis would be dismissed as irrelevant. From this perspective, a general consumer index is viewed solely in terms of its suitability as a proxy for the weighted average of the rates of change of replacement costs (of goods consumed in production) experienced by a business. However, this view is by no means universally accepted.<sup>8</sup> Thus an important second

line of the attack on general-index adjustments is the proposition that the purchasing patterns of individual users of accounts are too heterogeneous to be adequately proxied by a single index, i.e. that the heterogeneity hypothesis is true. Hence the hypothesis is worthy of testing.

For if rates of inflation vary greatly between individuals, then there would be good reason for doubting the usefulness of general-index adjustments. Indeed, if the rates vary greatly it would be difficult to envisage what the 'case' is which the Sandilands Committee has in mind in making the following recommendation:

We do not consider it is practical to adjust all the figures in the summary table by appropriate specific indices, but we think that there is a case for adjusting the successive figures for dividends paid by a specific index of the price changes experienced by shareholders, and we believe that the RPI is a sufficient approximation to such an index. In our view this is the only area in which the RPI has a useful role to play in accounting for inflation in the long term.<sup>9</sup>

This statement presupposes that the 'heterogeneity hypothesis' is false for shareholders, that rich and poor shareholders alike experience similar rates of inflation.

It should not be overlooked, of course, that some advocates of general purchasing power adjustments to (either historical cost or current value) accounts do not accept that the case for such adjustment would be invalidated by evidence of heterogeneity of personal rates of inflation; for example, Chambers<sup>10</sup> argues that price level measurements represent an aspect of the experiences of the whole community and not just those of individual members. Similarly, Moonitz argues that an index is needed which can serve '... as a surrogate for movements in the "terms of trade" between money and "the bundle of things in general" that money can buy'<sup>11</sup> and on this ground advocates the use of a GNP Implicit Price Deflator rather than a consumers' price index.<sup>12</sup>

Nevertheless, it seems to be implicit in most discussions of the uses to be made by an individual of price level adjusted accounting data that

<sup>7</sup>Sandilands Report, *op. cit.*, para. 46. For an indication of how this hypothesis may be tested, see Y. Ijiri, *Theory of Accounting Measurement*, Studies in Accounting Research No. 10, American Accounting Association, 1975, ch. 7.

<sup>8</sup>For example, see E. O. Edwards and P. W. Bell, *The Theory and Measurement of Business Income*, University of California Press, 1961; and more recently, W. T. Baxter, 'Accountants and Inflation', *Lloyds Bank Review*, October 1977, pp. 1-16.

<sup>9</sup>*Op. cit.*, para. 627. Emphasis added.

<sup>10</sup>R. J. Chambers, *Current Cost Accounting—A Critique of the Sandilands Report*, ICRA Occasional Paper No. 11, 1976, pp. 7-8.

<sup>11</sup>M. Moonitz, *Changing Prices and Financial Reporting*, ICRA Occasional Paper No. 3, 1973, p. 34.

<sup>12</sup>*Ibid.*, p. 37. Note, incidentally, that Moonitz reports (*ibid.*) a study over ten years ago of the behaviour of the American Consumers Price Index against that of the GNP Deflators. The two seemed to move more or less in tandem.



a general consumer index is an adequate proxy for the rates of inflation experienced by each user.<sup>13</sup> In this context, it is worthwhile noting that the finance literature is increasingly recognising the need to express investor benefits in real terms.<sup>14,15</sup> Furthermore, it is difficult to envisage what is meant by changes in the purchasing power of money in the absence of specifying whose purchasing power accountants have in mind. Keynes long ago argued this point:

We mean by the Purchasing Power of Money the power of money to buy the goods and services on the purchase of which for purposes of consumption a given community of individuals expend their money income... and the appropriate index-number is of the type sometimes designated as the Consumption Index. It follows that Purchasing Power must always be defined with reference to a particular set of individuals in a given situation namely those whose actual consumption furnishes us with our standard, and has no clear meaning unless this reference has been given.<sup>16</sup>

This viewpoint is echoed in paragraphs 44 to 49 of the Sandilands Report.

In view of recent developments in the accounting profession in the UK, the heterogeneity hypothesis cannot be regarded as a dead issue:

- (i) although the Sandilands Committee rejected both the CPP model of accounting as set out in SSAP7 and the inclusion of a monetary items adjustment in CCA financial statements, it still recommended the adjustment of dividends, as pointed out above;
- (ii) ED18 recommends the inclusion in financial reports of a statement showing the effects of change in the value of shareholders' equity, as reflected by the general index of retail prices;<sup>17</sup>
- (iii) a special meeting of members of the Institute of Chartered Accountants in England and

Wales on 6th July 1977 rejected ED18, thereby re-opening the debate on inflation accounting. The so-called 'Hyde Guidelines' only purport to be a stop-gap solution.

Far from being a dead issue, the case for general adjustments is still being vigorously put.<sup>18</sup> Hence the heterogeneity hypothesis, *inter alia*, still needs investigation.

### Measurement of personal rates of inflation

In order to test the heterogeneity hypothesis, it is necessary to agree upon a definition (or definitions) of how changes in costs of living should be measured.

It needs to be stated at the outset that there is no single generally accepted operational definition of changes in an individual's cost of living. Economists are agreed, though, that the true cost of living index for a given utility-maximising individual can be defined as the ratio of (i) the minimum cost of attaining either a reference indifference curve or a basket of goods at current prices to (ii) that of attaining the same under base time-point prices. Two broad approaches have been adopted by researchers in the field, the economic-theoretic approach and the statistical approach.

From a conceptual viewpoint, the economic-theoretic approach is the most satisfactory one: it defines 'well-offness' in terms of indifference. Unfortunately, the approach requires specification of a utility function;<sup>19</sup> the choice is arbitrary.

The statistical approach measures the variations in prices without employing a direct economic-theoretical underpinning. Governmental cost of living indices are of this type. Statistical indices can be classified into two main forms: (i) Laspeyres indices, where the price relatives are weighted by base period expenditures;<sup>20</sup> (ii) Paasche indices, where the price relatives are weighted by current expenditures.

In this article we have employed the statistical approach, mainly to avoid the problem of having to specify a utility function – but also because

<sup>13</sup>Put differently, this is the test demanded by critics of general-index adjustments.

<sup>14</sup>For example, see R. L. Hagerman and E. H. Kim, 'Capital Asset Pricing with Price Level Changes', *Journal of Financial and Quantitative Analysis*, September 1976; E. Fama, 'Short-term Interest Rates as Predictors of Inflation', *American Economic Review*, June 1975.

<sup>15</sup>Although this literature does not explicitly refer to the heterogeneity hypothesis, since the models are based on individual investor utility functions it seems to be implicit that the hypothesis is assumed to be false. This, it must be admitted, stems from the partial equilibrium character of the analyses, viewing inflation as a purely monetary influence, and therefore having but a 'scale' effect on individual investors.

<sup>16</sup>J. M. Keynes, *A Treatise on Money*, Vol. I: 'The Pure Theory of Money', Macmillan, 1930, p. 54.

<sup>17</sup>Accounting Standards Committee, *ED18: Current Cost Accounting*, 1977, paras. 120 to 125.

<sup>18</sup>See for example, E. Stamp, 'ED18 and Current Cost Accounting: A Review Article', *Accounting and Business Research*, Spring 1977, pp. 91–92; Baxter, *op. cit.*

<sup>19</sup>For example, see the various studies based on the Stone-Geary utility function, explained in J. R. N. Stone, 'Linear Expenditure Systems and Demand Analysis: An Application to the Pattern of British Demand', *Economic Journal*, September 1954, pp. 511–527 and R. C. Geary, 'A Note on "A Constant-Utility Index of the Cost of Living"', *Review of Economic Studies*, 1950, pp. 65–66.

<sup>20</sup>Strictly speaking, the only requirement of the Laspeyres index is that the weights be fixed through time. In normal practice, however, the weights are the expenditures of a past period.

more detailed analyses are possible with this approach.<sup>21</sup> (Economists, it should be noted, tend to favour the economic-theoretic approach because they are primarily interested in testing aspects of economic theory; thus studies showing that certain forms of constant-utility functions predict aggregate expenditure patterns very well<sup>22</sup> is evidence that more theoretically complete models which allow for changing preferences add little in terms of predictive ability.) Moreover it has been found that the statistical approach can give excellent approximations to the economic-theoretic approach.<sup>23</sup>

### Research design

Ideally this investigation should be focussed upon individuals (or households); a cost of living index should be calculated for each member of a sample of all individuals in the economy. However this is not a practicable proposition; data at the individual consumer level are not readily available. Information is available, though, on the expenditures of groups of individuals classified according to their money-income level. This is given in the annual *Family Expenditure Survey* (FES) published by the Central Statistical Office and is obtained from expenditure diaries of a large stratified sample of households (e.g. in 1974 6,695 households classified into 15 income groups).

Data on annual expenditures were extracted from the FES for the years 1961–1974 inclusive. The number of income-groups covered is shown below:

Years	Number of Income-Groups
1961–1966	9
1967–1972	12
1973	14
1974	15

In order to construct a cost of living index (COLI) for an income group, price data are

required on each item of group expenditure. The data should take the form of price relatives (rather than absolute prices),  $p_{it}/p_{is}$ , where  $p_{it}$  is the absolute price of good  $i$  at the end of period  $t$  and  $s$  is a fixed reference period ( $s < t$ ). For some purposes, the successive measurement of price relatives (i.e. the time interval  $t$  to  $t + 1$ ) should be as small as possible; for example, the adjustment for gains/losses on monetary items should be calculated ideally on a day to day basis (to get 'the area under the curve'). For others, e.g. the restatement of annual dividends, fluctuations within such small intervals are irrelevant.

The most detailed published data on consumer goods price relatives are the component indices of the *Retail Price Index*, reported on a monthly basis in the *Monthly Digest of Statistics*, covering Food, Drink, Tobacco, Housing, Fuel and Light, Durable Household Goods, Clothing and Footwear, Transport, Services, Miscellaneous. This monthly data forms the basis of the present study. Monthly rather than annual COLIs were constructed in order to maximise the likelihood of obtaining substantial between-group differences, i.e. of the heterogeneity hypothesis being true.

The methods used to calculate proportionate changes in the COLIs are as follows.

*Method I.* The 'fixed-weight' method. The approach taken here is to calculate a COLI based on expenditures of a base period and then to measure the month to month proportionate changes in the index. The index is defined as:

$$I_{gt}^I = 100 \sum_i \omega_{gib} (p_{it}/p_{io}) \quad (1)$$

where

$$\omega_{gib} = \frac{p_{ib} q_{gib}}{\sum_i p_{ib} q_{gib}} \quad (2)$$

such that

$$\sum_i \omega_{gib} = 1.$$

That is, the price relative of good  $i$  at the end of month  $t$  (i.e. the ratio of the price of  $i$  at  $t$  to its price at the reference period  $o$ ) is weighted by the proportion spent on  $i$  by group  $g$  in base period  $b$ .

The next step in the analysis is to take proportionate changes in the index  $I_{gt}^I$ , denoted  $R_{gt}^I$ :

$$\begin{aligned} R_{gt}^I &= \frac{I_{gt}^I - I_{g,t-1}^I}{I_{g,t-1}^I} \\ &= \sum \omega'_{gib} (p_{it}/p_{i,t-1}) - 1 \end{aligned} \quad (3)$$

<sup>21</sup>This point can be more readily understood if we consider the work of J. Muellbauer, 'Prices and Inequality: The U.K. Experience', *Economic Journal*, March 1974, pp. 32–55. This study made the assumptions, *inter alia*, that different households had utility functions of the same form and that the parameters remained constant across households and through time. It was not possible with this approach to categorise expenditure as finely as we have done in our study.

<sup>22</sup>For example, the estimates in Belgium of 1970 consumer expenditure for 8 broad classes of goods were all within 10% of the observed expenditures. See R. Sanz-Ferrer, 'Prévisions de la consommation privée en Belgique', *Recherches Économiques de Louvain*, 1972, pp. 17–37, cited in L. Philips, *Applied Consumption Analysis*, North-Holland, 1974.

<sup>23</sup>For example, the Laspeyres index is a very good proxy for the corresponding economic-theoretic index (the so-called static true index); see Philips, *ibid.*, pp. 138, 233.

where

$$\omega'_{gib} = \frac{p_{i,t-1} q_{gib} (p_{ib}/p_{io})}{\sum_i p_{i,t-1} q_{gib} (p_{ib}/p_{io})} \quad (4)$$

*Method II.* 'Fixed-weighting of month to month price relatives' method. The approach taken here is to calculate directly period to period changes in group *g*'s cost of living—rather than first calculating a COLI and then taking period to period proportionate changes, as in method I. The proportionate change is

$$R_{gt}^{II} = \sum_i \phi_{gib} (p_{it}/p_{i,t-1}) - 1 \quad (5)$$

where

$$\phi_{gib} = \frac{p_{ib} q_{gib}}{\sum_i p_{ib} q_{gib}} \quad (6)$$

It should be noted that neither Method I nor Method II corresponds exactly to a Paasche or Laspeyres index. Neither a Paasche nor a Laspeyres index could be calculated since this requires separate price and quantity data and the latter are not available.

Consider the form of period to period proportionate changes in methods I and II. It can be shown that proportionate changes in a Laspeyres take the form

$$R_{gt}^L = \sum_i \theta_{gib} (p_{it}/p_{i,t-1}) - 1 \quad (7)$$

where

$$\theta_{gib} = \frac{p_{i,t-1} q_{gib}}{\sum_i p_{i,t-1} q_{gib}} \quad (8)$$

It can be seen that equations (3), (5) and (7) have the same form: they are weighted averages of month to month price relatives. They differ only in the composition of the weights. Comparing (4) and (8), the weights in Method I differ from those of the Laspeyres by a price relative  $(p_{ib}/p_{io})$ .<sup>24</sup> To the extent that this price relative is constant for the *i* goods (i.e.,  $p_{ib}/p_{io} = p_{i+s,b}/p_{i+s,o}$  for all *s* ≠ 0),<sup>25</sup> Method I will approximate a Laspeyres.

On the other hand, a comparison of (6) and (8) indicates that Method II is of a different character to the Laspeyres form: in the former

the weights  $\phi_{gib}$  are constant from period to period, whereas in the Laspeyres the weights  $\theta_{gib}$  change each period. Similarly, it can be shown for the Paasche that the weights on the period to period price relatives  $p_{it}/p_{i,t-1}$  also change each period, due not only to price changes (as in the Laspeyres) but also to quantity changes. Method II, on the other hand, attempts to hold both price and quantity constant in the weights. The purpose of Method II is to maximise the likelihood of between-group differences; when prices are rising consumers will tend to substitute goods whose prices are rising slowly for those whose prices are rising quickly, thereby tending to equalise the impact of inflation: Sterling's playboy *may* make a greater proportionate reduction in his wine expenditure if wine prices rise relatively fast than the worker who already consumes little wine. That is, Method II does not incorporate the dampening effect of substitution.

*Method III.* The 'moving fixed weight' method. The approach taken here is similar to Method I in that the COLI is a weighted average of price relatives, as in equation (1). It differs in that the weights change from year to year; for example the price relatives in 1967 are weighted by the income group's proportionate expenditures in 1967. Thus, in equations (2) and (3) the base period *b* is revised annually, i.e.

$$\begin{aligned} \text{for } t = 1, 2, \dots, 12, \quad & b = 1962 \\ \text{for } t = 13, 14, \dots, 24, \quad & b = 1963 \end{aligned}$$

and so on.

The moving fixed weight index employed in Method III is similar to the Paasche index in that the quantities  $q_{gib}$  in (2) are up-dated annually. The weights used in the Retail Price Index (RPI) published by the Government Statistical Service are also up-dated annually, in analogous fashion. To the extent that Method I approximates a Laspeyres, Method III approximates a moving Laspeyres.

*Method IV.* The 'moving fixed weighting of period to period price relatives' method. This approach is similar to method II in that month to month changes are calculated directly as in (5), except that the weights change from year to year in the same manner as shown above for Method III.

The month to month proportionate changes in the cost of living for each of the income groups were calculated for the period 1962–1974 by methods I and II, and for the period 1962–1972 by methods III and IV, using the fourteen different base-expenditure years 1961–1974.

<sup>24</sup>It is identical to the Laspeyres when  $b = 0$  in equation (4).

<sup>25</sup>See the following section for evidence on this point.

The next stage of the analysis is to determine the between-groups commonality in the proportionate changes. Three methods suggest themselves, pairwise correlation, principal components and regression.

Pairwise comparison would indicate the extent to which one income group's cost of living moved in step with another's. Strictly speaking, however, the issue is not whether one group's indices could be used in place of another's, but whether all the groups' indices could be adequately proxied by a weighted combination of them; this is given by the correlations between the individual group indices and the weighted total index. For we are not concerned with the extent to which one of the indices is a surrogate for any of the others, but rather with the adequacy of a 'total' index as a proxy for each of them.

Note that we are not concerned with any currently available total index such as the RPI (or the Gross Domestic Product Implicit Price Deflator), but only with the commonality amongst the group indices. We are not addressing the issue of whether a currently reported total consumer index reflects the changes in the purchasing power of individuals; the more fundamental question here is whether or not personal rates of inflation are (more or less) the same. Relating proportionate changes in group  $i$ 's index to proportionate changes in, say, the RPI would confound the two separate issues of: (a) the amount of commonality amongst the group indices; and (b) the extent to which a particular total consumer index provides an accurate measure of the commonality, i.e. approximates the best 'total' index.

An attractive feature of principal components analysis is that it generates such a best total index, one that captures the maximum amount of variation amongst the group indices. The logic of the principal components method is explained in Harman<sup>26</sup> and Kendall.<sup>27</sup> The aim of the method is to construct out of a set of  $G$  variables (in this study each variable representing the monthly proportionate change in living costs of a particular income group) a new set of variables capturing all the variability of the original set such that the new variables are (i) uncorrelated with each other (orthogonal) and (ii) ordered according to the proportion of variability thus captured. From our viewpoint the interesting issue is the extent to which the original  $G$  variables can be proxied by the first new variable

(the first principal component); this is given by (i) the ratio of the largest eigenvalue  $\lambda_1$  to the sum of the eigenvalues

$$\sum_{g=1}^G \lambda_g$$

and (ii) the eigenvector associated with  $\lambda_1$ . The eigenvalues are the characteristic roots of the matrix of covariances (about the origin) between the  $G$  groups' proportionate changes in costs of living. The  $g^{\text{th}}$  element in the eigenvector associated with  $\lambda_1$  gives the correlation between the first principal component and the  $g^{\text{th}}$  old variable (the change in  $g$ 's cost of living).<sup>28</sup>

Intuitively, what the principal components method does is the following. As part of the procedure, a fixed set of  $G$  weights are generated, which for any particular month can be used to calculate a weighted mean of the  $G$  groups' change in cost of living. This mean may then be compared to each and all of the  $G$  changes for that month, thus allowing the estimation of the dispersion around the mean; this exercise is done for each of the months. The  $G$  weights are chosen so as to minimise the overall dispersion; i.e. the weights chosen are 'optimal' or 'best' in the sense of ensuring the calculation of the 'most representative' index of the monthly changes (the first principal component, referred to above). From this it is possible to calculate the goodness of fit of this best index and also the  $G$  correlations of the individual indices with the best index.

Another way of estimating the amount of commonality amongst the group indices would be to regress proportionate changes in the group indices on a published total consumer index. This has the problem though of confounding commonality with surrogation of commonality, referred to above, and has therefore not been employed in this study.

## Empirical results

Table 1 provides summary information concerning the overall goodness of fit of the best index, using methods I and II described above. Goodness of fit is defined as the amount of variability captured by the first principal component i.e. the

ratio of  $\lambda_1$  to  $\sum_{g=1}^G \lambda_g$ .

<sup>26</sup>H. Harman, *Modern Factor Analysis*, 2nd ed., University of Chicago Press, 1967.

<sup>27</sup>M. G. Kendall, *Multivariate Analysis*, Griffin, 1976.

<sup>28</sup>For a more detailed explanation of this method in the context of a related accounting problem, see K. V. Peasnell and L. C. L. Skerratt, 'How Well Does a Single Index Represent the Nineteen Sandilands Plant and Machinery Indices?', *Journal of Accounting Research*, Spring 1977.

**Table 1**

**Statistics Associated with the First Principal Component (PCI) Derived from Monthly Proportionate Changes in Groups' Costs of Living, 1962-1974**

Number of Groups	Weights used	% captured by PCI	
		Method I	Method II
9	1961	98.81	99.98
9	1962	98.69	99.88
9	1963	98.68	98.64
9	1964	98.80	98.77
9	1965	98.63	99.98
9	1966	98.79	98.75
12	1967	98.68	98.64
12	1968	98.79	98.75
12	1969	98.95	98.91
12	1970	99.20	99.19
12	1971	99.18	99.16
12	1972	99.03	99.00
14	1973	99.07	99.03
15	1974	99.13	99.10

In all cases the amount of variability captured by the first principal component (for brevity PCI) is in excess of 98%, indicating that the groups' inflation rates exhibit a considerable degree of commonality. The correlations between PCI and the changes in the costs of living of each of the G groups are given in Tables A.1 and A.2 in the Appendix; it can be seen that they are all in excess of 97%.

The results of using methods III and IV are shown in Table 2. Comparing Tables 1 and 2, it can be seen that the effect of using current weights is to produce measures of group inflation rates which are *slightly* more heterogeneous. An examination of the eigenvectors in Table A.3 reveals that for the period 1962-1966 method III's poorer result is due primarily to the seventh highest income group; the correlation between this and PCI is 0.85, whereas the remainder are 0.95 or higher. Overall, it is quite clear that the fit is extremely high.

It should not be overlooked, of course, that these results may be due to the high degree of aggregation employed in this study. The price data used, the ten sub-groups of the Retail Price Index, are highly aggregated indices. It may be that this level of aggregation is so high as to eliminate much of the underlying differences in the price changes of goods consumed; that is, important variations in the prices of goods may be *within* a given subgroup rather than between subgroups. In order to test this the first step was to examine the price data for the ten subgroups of the RPI, the approach taken being to extract the first principal component from the variance-covariance matrix of month to month proportionate changes in the ten sub-groups; it was found that this first principal component captured only 42.8% of the total variability. In addition, an examination of the associated eigenvector given in Table A.4 indicates considerable dispersion - Fuel and Light, in particular, is weakly cor-

**Table 2**

**Statistics Associated with the First Principal Component Derived from Monthly Proportionate Changes in Groups' Costs of Living, 1962-1972, Using Current Weights**

Number of Groups	Period Covered	% Captured by PCI	
		Method III	Method IV
9	1962-1966	92.69	97.28
12	1967-1972	97.43	98.83



related with the first principal component (0.27). These results strongly suggest that there is considerable between-subgroup variation,<sup>29</sup> although the possibility of considerable within-subgroup variability cannot be dismissed.

It should be noted that the most deviant subgroups, Fuel and Light, Food, are *not* lightly weighted in methods I–IV; Food for example is, for all income groups, the most important item of expenditure, and yet it is weakly correlated (0.55) with the first principal component. Hence it cannot be argued that the choice of price-aggregation scheme has resulted in a domination by highly correlated sub-groups. What is clear, though, is that there is a need to examine the effects of within-subgroup price variations. To this end, Food was selected for further study—mainly because it is the only category of expenditure for which detailed price data are available, but also because of its weak correlation with other subgroups and its importance in family budgets (from one quarter to one third of expenditures).<sup>30</sup>

Average price quotations covering 76 food categories are reported monthly in the *Department of Employment Gazette*. These data form the basis of income group food cost of living estimates reported below. These were calculated in order

to get some idea of the effects on the costs of living of various income groups of within-subgroup price variation.

Unfortunately, income groups' weight data on food are less finely partitioned than the price data. Consequently, the 76 food categories were condensed to 20 by arithmetic averaging of price relatives. An indication of the resultant information loss can be obtained by comparing the proportion of variability captured by the first principal component based on the 76 items with that based on the 20 items—29.2% and 57.3%, respectively.<sup>31</sup> Though the information loss is considerable, the proportion still captured (57.3%) is not so high as to render meaningless the comparison of income groups' food inflation rates.

The income groups' food inflation rates were calculated according to methods I and II for the months of 1972–1975. (This was the longest period for which comparable price data were available.) The results are given in Table 3. It can be seen that the degree of commonality amongst income groups is very high indeed; this is supported by an examination of the eigenvectors given in Table A.5. These results suggest that, for food at least, within group price variation has an insignificant effect on rates of change of costs of living.

Next a test was made of the extent to which the calculation of monthly proportionate changes in groups' total costs of living by method I approximates a Laspeyres. This point was discussed in the preceding section, where it was pointed out that to the extent that the price relative  $p_{it}/p_{i0}$  in equation (4) is the same for all  $i$ , method I approximates a Laspeyres. To this end the first principal component was calculated from the cor-

<sup>29</sup>In a different context, viz. the commonality amongst stock market price changes, it is worth noting that B. J. King ('Market and Industry Factors in Stock Price Behavior', *Journal of Business*, January 1966) found that the typical stock has about half of its variance explained by an element of price change which affects the whole market. King's investigation was at the individual security level, and suggests a certain degree of commonality amongst price changes is a widespread economic phenomenon. It is therefore not just a feature of the data set used in the present study.

<sup>30</sup>This has been borne out by another study: 'Food turns out to be the strategic commodity: a 25% rise in the relative price of food would wipe out half of the measured (uncorrected) decline in real inequality...' (emphasis in original). Muellbauer, *op. cit.*, p. 46.

<sup>31</sup>The associated eigenvectors are not reported in the Appendix as there is no point in examining the individual correlation coefficients.

**Table 3**  
**Statistics Associated with the First Principal Component Derived from Monthly Proportionate Changes in Groups' Food Costs of Living, 1972–1975**

Number of Groups	Weights Used	% Captured by PCI	
		Method I	Method II
12	1971	99.70	99.68
12	1972	99.70	99.62
14	1973	99.63	99.63
15	1974	99.67	99.65
16	1975	98.38	98.07

relation matrix of monthly *levels* of the ten sub-groups of the Retail Price Index.<sup>32</sup> 97.5% of their total variability was captured, suggesting that these price relatives do move in step and therefore that Method I gives a good approximation to the Laspeyres.

The doubt must remain that the income-group measures of inflation reported above may be too crude to provide an adequate test of the heterogeneity hypothesis. It may be that important differences between groups have not been picked up, especially in the case of groups at the extreme ends of the income spectrum.

The only other empirical study of which we are aware is that by Muellbauer<sup>33</sup> who has estimated, by different methods and with different data (extracted from the Blue Book), annual cost of living indices for 9 different income<sup>34</sup> groups for the period 1964–1972. His estimation procedures assume that all groups have the same utility function, one in which the parameters remain constant through time.

Although Muellbauer's conclusions are hedged about with a number of reservations, he is of the general opinion that his 'study confirms that for more than twenty years relative consumer price changes have had an inegalitarian bias and suggests that the degree of bias has recently been increasing'.<sup>35</sup> This may be so, but an examination of his results indicates that the bias is quite small: the difference between the *levels* of the cost of living index for the highest and lowest groups was only 4.6% after 8 years (144.7 to 151.4 at 1972, 100 at 1964); the maximum absolute dispersion between the geometric means of the annual rates of change of the indices (5.317% minus 4.719%) is 0.598%, and as a proportion, 12.7%. These, it should be noted, are the differences between the outliers; when compared to 'normal' income groups the differences are much smaller. To test this we computed the amount of variability captured by the first principal component<sup>36</sup> and found that 99.6% of the variability in Muell-

bauer's indices was thus accounted for and that all the correlation coefficients in the associated eigenvector were in excess of 99% (not reported), which is in line with our own findings.

It has to be admitted, of course, that both Muellbauer's measures of cost of living changes and ours involve very high levels of aggregation, whereas government statisticians have far more information available to them for the construction of income group indices, if they have a mind to. Apart from general indices, such as the RPI, the only income-group indices published by the government are quarterly figures for the 'one-person pensioner households' RPI and the 'two-person pensioner households' RPI. Given that pensioners in general tend to be found towards the lower end of the income spectrum, the extent of the co-movement of these two indices and the total RPI provides another useful cross-check on our own results. To test this we computed in the usual way the amount of variability captured by the first principal component for the 3 indices: it accounted for 98%, which is again in line with our own findings.

### Concluding remarks

This paper reports an empirical test of the hypothesis that the rate of inflation (proportionate changes in living costs) varies markedly between income-groups. In the absence of any robust definition of personal inflation, one which would dominate all others in all circumstances, an eclectic approach utilising a number of different definitions is adopted. A remarkable degree of commonality amongst income-group inflation rates can be observed, for all the various bases employed. On the basis of these results, the heterogeneity hypothesis must be rejected.

No doubt this result will come as a surprise to many readers. It has been put to us that it is contrary to common sense. We contest this on the following grounds.

Firstly, it is a fact that widespread references are made in labour negotiations, press commentaries, and the like to consumer indices. As Bromwich points out:

A cursory glance at the world suggests that many people do find general purchasing power

<sup>32</sup>The correlation, rather than the variance-covariance, matrix is the relevant way of representing variability here, since index levels (unlike proportionate changes) are not dimensionless and therefore need to be normalised. See K. V. Peasnell and L. C. L. Skerratt, 'Price Indices for Current Cost Accounting—A Reply and Some Further Evidence', *Journal of Business Finance and Accounting*, Spring 1977, p. 141, and F. H. C. Marriott, *The Interpretation of Multiple Observations*, Academic Press, 1974, pp. 19–20, for more details.

<sup>33</sup>*Op. cit.*

<sup>34</sup>More precisely, he defines groups according to weekly expenditures made in 1964.

<sup>35</sup>Muellbauer, *op. cit.*, pp. 45–46.

<sup>36</sup>Derived in the same way as in our own study, i.e. using the variance-covariance matrix around the origin for the annual proportionate changes in Muellbauer's group cost of living indices.

measurement systems useful for many different purposes.<sup>37</sup>

Secondly, it is important to recognise that the effects of price variations on an individual's cost of living can be viewed from *both* partial and total standpoints. The fact (if it is a fact) that one individual's rate of inflation is more or less in step with everybody else's does not rule out the possibility that his expenditure patterns diverge sharply from theirs, and that some of his heavily-weighted expenditures have price movements at odds with those of other goods. That is, the doubling of the price of coffee will have a pronounced *marginal* impact on the heavy coffee drinker's cost of living and none at all on those who do not imbibe; but the *total* impact may be trivial due to other influences, such as cross-correlations in price movements of goods, substitution and the like. The distinction between total and marginal impact is akin to the distinction between partial and total differentiation in mathematics.

The fact of the matter is that all but the most deviant (sociologically speaking) of individuals spend their money on a very wide range of goods, albeit in different proportions. There is therefore bound to be a dampening effect which tends to average out the marginal impact of outliers. Furthermore, if one price has a tendency to rise faster because of supply influences (e.g. coffee), demand effects will tend to dampen down the increase. The onus must now surely be on the believers in the heterogeneity hypothesis to demonstrate that this is not so.

There remains the possibility that the results obtained in this paper are a direct function of imperfections in the data employed, that too much averaging has occurred in compiling the price movements. This may indeed be the case with the tests based upon the subgroups of the Retail Price Index; but it is most unlikely to apply to the tests on items of food, or to the tests on pensioner indices, which were both based on published quotes of actual prices—and the results were the same in all three cases.

It is important to recognise that our results do not constitute a test of the hypothesis that the rate of inflation varies markedly for different *individuals*; the hypothesis tested in this study is that the rate of inflation varies markedly for different *income-groups*. The possibility of within-income-group heterogeneity of inflation rates cannot be

ruled out. Unfortunately, data at the individual consumer level are not readily available. It would be useful if subsequent research on this topic were to be conducted at the individual consumer level.

It could be argued that more significant differences would have been detected if the groupings had been done by geographical region rather than by income since regional differences in price movements might be more important. Unfortunately, data on prices ruling in different areas are not available; hence the only differences that could have been picked up would have been variations in regional expenditure patterns. These geographical effects remain to be investigated.

The prior belief on which our study was based was that income was likely to be the most important determinant of expenditure patterns. Studies have shown that at an aggregate level income differences account for a substantial amount of the variations in expenditure patterns.<sup>38</sup> Furthermore, an examination of our *Family Expenditure Survey* data on the proportions spent on various expenditure categories during the period 1961–1974 indicates that there is indeed a strong 'income effect'. Thus in 1961 the lowest income-group spent 37% of its budget on food, whereas the highest income-group spent only 23%; by 1974, these proportions had fallen to 33% and 19% for the lowest and highest income-groups respectively. This is in accordance with what economic theory leads us to expect: 'Engel's Law' suggests that the % of income spent on food declines as income increases.<sup>39</sup> Hence grouping UK individuals according to their income seemed likely to minimise information losses from aggregation. However our results indicate that income-induced differences in expenditure patterns do not seem to be accompanied by corresponding variations in rates of inflation. It remains an open question as to whether another basis of aggregation (or treating the subject at the individual consumer level) would give different results.

We have tried to estimate the degree of commonality amongst rates of inflation experienced *in the past*; there is no guarantee that the observed high degree of common movement will recur in the future. In particular, it should be

<sup>37</sup>M. Bromwich, 'Individual Purchasing Power Indices and Accounting Reports: A Comment on a Suggestion by Professor Gynther', *Accounting and Business Research*, Spring 1975, p. 120.

<sup>38</sup>J. R. N. Stone and D. A. Rowe, 'The Market Demand for Durable Goods', *Econometrica*, July 1957, pp. 423–443; S. J. Prais and H. S. Houthakker, *The Analysis of Family Budgets*. Cambridge University Press, 1955.

<sup>39</sup>H. S. Houthakker, 'An International Comparison of Household Expenditure Patterns Commemorating the Centenary of Engel's Law', *Econometrica*, October 1957, pp. 532–551.



noted that in times of high inflation (such as Britain has recently experienced) all prices tend to move up; this 'inflation effect' doubtless accounts for much of the common movement in the underlying consumer price series. By definition this inflation effect would not be so important in periods of low general inflation; personal inflation rates *might* be more heterogeneous. However when inflation is negligible the case for the general-index adjustment of accounting reports is nowhere nearly so strong as when inflation is raging. The high degree of common movement in personal rates of inflation reported in this article seems likely to repeat itself when it most matters, in periods of high inflation.

In this paper no attempt is made to evaluate the theoretical merit of general-index adjustments. Rather the purpose is to determine whether or not the experience of inflation is so different for different individuals as to render such adjustments meaningless. As shown by the results of this study, the impact of inflation is remarkably constant across income groups, suggesting that the concept of general inflation is meaningful to different individuals.

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## Appendix

The eigenvector associated with the largest eigenvalue (from which is calculated the first principal component) shows the amount of association between the first principal component and *each* of the income group indices; the  $i^{\text{th}}$  element in the eigenvector gives the correlation coefficient between the first principal component and the index for group  $i$ .

It is to be expected that the rates of inflation experienced by the highest and lowest income groups will tend to diverge from the general experience because these groups' expenditure patterns are likely to be different from the average; in which case, the correlation coefficients will be lower for these groups. Reading down any particular column of the tables below, we would expect the coefficients to rise, flatten out and then decline; this is indeed what tends to happen – although the curve is very flat suggesting that the rates of inflation experienced by the extreme groups are remarkably similar to the average.

**Table A.1**  
**Eigenvectors Associated with the First Principal Components in Table 1, Method I, Showing the Coefficients between the Individual Income-Group Indices and the Constructed General Index**

Income Group Nos.*	Weight Years														
	1961	1962	1963	1964	1965	1966	1967	1968	1969	1970	1971	1972	1973	1974	
1	0.986	0.984	0.980	0.976	0.980	0.980	0.983	0.986	0.991	0.984	0.982	0.981	0.983	0.991	
2	0.991	0.984	0.986	0.984	0.990	0.992	0.988	0.984	0.985	0.993	0.993	0.991	0.988	0.986	
3	0.997	0.988	0.985	0.992	0.998	0.998	0.992	0.994	0.993	0.998	0.998	0.997	0.995	0.994	
4	0.999	0.997	0.993	0.988	0.999	0.999	0.985	0.997	0.997	0.999	0.999	0.999	0.999	0.997	
5	0.999	0.989	0.993	0.988	0.999	0.999	0.999	0.999	0.999	0.999	0.999	0.999	0.999	0.999	
6	0.999	0.992	0.989	0.998	0.999	0.998	0.998	0.999	0.999	0.999	0.999	0.999	0.999	0.999	
7	0.994	0.987	0.993	0.996	0.994	0.996	0.998	0.998	0.998	0.999	0.999	0.999	0.999	0.999	
8	0.993	0.984	0.984	0.996	0.990	0.994	0.998	0.997	0.997	0.999	0.999	0.999	0.999	0.999	
9	0.983	0.983	0.978	0.987	0.991	0.990	0.996	0.996	0.997	0.998	0.998	0.998	0.998	0.999	
10							0.994	0.996	0.995	0.996	0.997	0.996	0.998	0.998	
11							0.993	0.992	0.995	0.996	0.995	0.995	0.998	0.998	
12							0.984	0.989	0.989	0.993	0.992	0.989	0.996	0.996	
13													0.995	0.996	
14													0.990	0.994	
15														0.990	

\*NOTE: Groups are arranged in order of increasing income: 1 refers to the lowest-income group, 2 to the next lowest, and so on.

\*NOTE: Groups are arranged in order of increasing income: 1 refers to the lowest-income group, 2 to the next lowest, and so on.

**Table A.2**  
**Eigenvectors Associated with the First Principal Components in Table 1, Method II, Showing the Correlation Coefficients between the Individual Income-Group Indices and the Constructed General Index**

Income Group Nos.*	Weight Years														
	1961	1962	1963	1964	1965	1966	1967	1968	1969	1970	1971	1972	1973	1974	
1	0.999	0.995	0.989	0.982	0.998	0.978	0.981	0.985	0.990	0.983	0.981	0.979	0.981	0.990	
2	0.999	0.993	0.986	0.989	0.999	0.991	0.987	0.982	0.984	0.992	0.992	0.991	0.987	0.984	
3	0.999	0.999	0.995	0.998	0.999	0.998	0.992	0.994	0.992	0.997	0.997	0.996	0.994	0.993	
4	0.999	0.998	0.999	0.998	0.999	0.999	0.997	0.996	0.997	0.999	0.999	0.999	0.998	0.996	
5	0.999	0.999	0.998	0.998	0.999	0.999	0.999	0.999	0.999	0.999	0.999	0.999	0.999	0.999	
6	0.999	0.999	0.995	0.998	0.999	0.997	0.998	0.998	0.999	0.999	0.999	0.999	0.999	0.999	
7	0.999	0.999	0.993	0.995	0.999	0.995	0.998	0.998	0.998	0.998	0.999	0.998	0.998	0.999	
8	0.999	0.999	0.992	0.995	0.999	0.994	0.997	0.996	0.997	0.998	0.998	0.999	0.999	0.998	
9	0.999	0.998	0.987	0.987	0.999	0.989	0.996	0.995	0.997	0.998	0.998	0.997	0.998	0.999	
10							0.993	0.995	0.995	0.995	0.997	0.996	0.998	0.998	
11							0.992	0.992	0.994	0.995	0.995	0.994	0.997	0.997	
12							0.984	0.989	0.988	0.992	0.992	0.989	0.995	0.995	
13													0.995	0.993	
14													0.990	0.993	
15														0.990	

\*NOTE: Groups are arranged in order of increasing income: 1 refers to the lowest-income group, 2 to the next lowest, and so on.

\*NOTE: Groups are arranged in order of increasing income: 1 refers to the lowest-income group, 2 to the next lowest, and so on.

**Table A.3**

**Eigenvectors Associated with the First Principal Components in Table 2 Showing the Correlation Coefficients between the Individual Income Group Indices and the Constructed General Index**

<i>Income Group Nos.*</i>	<i>Method III</i>		<i>Method IV</i>	
	1962-1966	1967-1972	1962-1966	1967-1972
1	0.952	0.962	0.962	0.983
2	0.982	0.980	0.981	0.987
3	0.986	0.980	0.997	0.994
4	0.991	0.992	0.998	0.996
5	0.991	0.997	0.996	0.998
6	0.991	0.995	0.993	0.998
7	0.855	0.995	0.989	0.997
8	0.969	0.996	0.985	0.997
9	0.978	0.995	0.974	0.995
10		0.993		0.993
11		0.985		0.991
12		0.976		0.988

\*NOTE: Groups are arranged in order of increasing income: 1 refers to the lowest-income group, 2 to the next lowest, and so on.

**Table A.4**

**Statistics Summarising the Extent to which the Component Price Indices of the Retail Price Index Covary, 1962-1974 Monthly**

*Proportion of Variability Captured  
by the Largest Eigenvalue*

0.428

*Eigenvector*

Food	0.554
Drink	0.882
Tobacco	0.785
Housing	0.719
Fuel & Light	0.273
Durable Household Goods	0.888
Clothing & Footwear	0.846
Transport & Vehicles	0.892
Miscellaneous	0.844
Services	0.654

**Table A.5**

**Eigenvectors Associated with the First Principal Components in Table 3, showing the Correlation Coefficients between the Individual Income-Group Indices and the Constructed General Index**

<i>Income Group Nos.*</i>	<i>Method I</i>					<i>Method II</i>				
	1971	1972	1973	1974	1975	1971	1972	1973	1974	1975
1	0.994	0.990	0.994	0.995	0.808	0.989	0.989	0.991	0.994	0.800
2	0.996	0.994	0.994	0.992	0.991	0.997	0.998	0.994	0.993	0.990
3	0.998	0.997	0.997	0.998	0.993	0.999	0.998	0.997	0.996	0.993
4	0.999	0.998	0.998	0.998	0.997	0.999	0.999	0.995	0.997	0.998
5	0.997	0.996	0.996	0.996	0.997	0.998	0.998	0.995	0.999	0.998
6	0.995	0.998	0.998	0.997	0.995	0.998	0.998	0.996	0.996	0.998
7	0.997	0.995	0.995	0.997	0.995	0.998	0.999	0.995	0.994	0.995
8	0.999	0.999	0.999	0.996	0.999	0.999	0.997	0.998	0.996	0.997
9	0.999	0.996	0.996	0.996	0.998	0.999	0.999	0.995	0.996	0.996
10	0.999	0.998	0.998	0.998	0.996	0.999	0.999	0.996	0.995	0.998
11	0.998	0.997	0.997	0.996	0.998	0.999	0.999	0.996	0.994	0.995
12	0.993	0.996	0.996	0.997	0.996	0.995	0.995	0.996	0.995	0.997
13			0.996	0.996	0.998			0.998	0.997	0.996
14			0.992	0.995	0.996			0.991	0.996	0.995
15				0.992	0.997				0.993	0.998
16					0.997					0.999

\*NOTE: Groups are arranged in order of increasing income: 1 refers to the lowest-income groups, 2 to the next lowest, and so on.

## ACCOUNTING, ORGANIZATIONS AND SOCIETY

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# Comment on Value Accounting for Currency Transactions

W. J. Brennan

As I understand the thesis of Earl and Paxson's 'Value Accounting for Currency Transactions' (*Accounting and Business Research*, Spring 1978), it is proposed that a transaction in a foreign currency should be recorded using the forward rate in respect to the date of expected settlement. The reasons presented in support of this view are as follows:

- (a) to eliminate some 'doubtful' exchange gains and losses from the accounts,
- (b) to achieve present realistic values for currency amounts, and
- (c) to create currency awareness by management.

In my view the case has not been made. The 'doubtful gains and losses' are indeed just the items to create full 'currency awareness' but in the 'present realistic value' two aspects of the transaction have been confused, the purchase or sale of goods on the one hand and the financing of the transaction on the other.

## Net price or gross price

The issue can be explored using a fairly simple down to earth bookkeeping situation (i.e., no exotic foreign exchange dealings). The example is the accounting for the following transaction:

a purchase of a box of widgets for a stated price of £1,000 subject to a 2% cash discount if paid within 10 days, but finally due in 30 days.

There are many ways to reflect this transaction in the accounting records but intermediate accounting textbooks cite two general methods, the net price or the gross price method.

**Net Price Method.** Under the net price method the asset 'stock' is debited at the net price (cash price on day of acquisition) of the widgets, i.e., £980. If payment is made within the discount period the cash paid equals the initially recorded liability, i.e., £980. If payment is made after the discount period any incremental payment (e.g.,

the £20 if paid prior to the thirty day due date) is considered as essentially an interest cost on the outstanding payable.

**Gross Price Method.** Under the gross price method the asset 'stock' is debited at the gross price (i.e., £1,000). If payment is made within the discount period the discount is accounted for as such and presented variously as 'other revenue', as 'an offset to the cost of stock purchased', or as a part of the financing charges section of the income statement. If payment is made after the discount period payment equals the recorded liability and no further accounting is necessary.

The choice of these methods is made on a number of bases. However, the net price method is generally supported on conceptual grounds whereas the gross price method is generally supported on practical application grounds. The net price method clearly distinguishes the aspect of the transaction which is associated with acquiring an asset (i.e., the widgets) from the aspect of the transaction which involves the financing of the investment. To reflect the cost of the assets we use a cash price, a price which will be identical no matter which financing procedure is used. The costs associated with financing are accounted for separately.

Use of the net price method will always highlight the discounts not taken. Use of the gross price method will highlight the discounts actually taken. Whether it is planned (expected) or not the act of not taking the discount is a decision to pay interest in exchange for additional time to pay for the goods. The fact of planning not to take the discount does not alter the nature of the payment.

On the basis of this example I suggest that the 'loss' is not doubtful and moreover it is the presentation of the (planned or unplanned) discounts not taken which will stimulate 'discount awareness'. This leads me to advocate, on conceptual grounds, the use of the net price method for accounting for the acquisition of stock.

The gross price method is convenient for processing invoices in an accounting system and, particularly if cash discounts are not taken, allows very simple bookkeeping procedures. It can be advocated and condoned only on that basis.

### Foreign transactions

In the simple situation of a one shot foreign transaction which is fully and specifically hedged it may be argued that economy demands that the accounting system simply record the transaction using the forward rate. This would be based on much the same argument used to defend the gross price method. However, application of the same logic used above to this situation would indicate that the spot rate (cash price) on the day of recording is the (conceptually) appropriate methodology. The rule in question is aimed not at the relatively simple situation, but at the vast majority of concerns where the complexity of the situation demands conceptually sound measurement systems not pragmatic band aids.

I fail to see how, even in my simple situation, use of the forward rate promotes currency risk awareness. On the contrary I submit that to record at the spot rate enables, even requires, the reporting system to recognize separately (a) the gain or loss on exchange rate fluctuation (i.e., spot rate to spot rate movements) as a result of the monetary item and (b) the gain or loss resulting from the undertaking of a forward contract. Surely the reporting of such information would stimulate among managers a concern for 'anticipation of exchange gains and losses'. The net difference between (a) and (b) can be seen as a measure of the effectiveness of this anticipation.

In the simple completely hedged situation the two categories of gains or losses would offset each other. However, in the vast majority of situations these two types of gains and losses would highlight risks incurred, their potential effect and the impact of steps taken, or not taken, to counteract such effects. This certainly would result in more 'awareness' than would result if the spot to spot fluctuation were submerged in use of the forward rate.

There are a number of other aspects of the article that deserve questioning. The enumeration of ways used to estimate the 'spot' rate as well as the suggestion that some companies use the 'expected rate' begs to be substantiated by empirical investigation.

In the analysis of the meaningfulness of exchange gains and losses an example of a 'dys-

functional consequence' is cited. The example is very difficult to follow:

(a) If we are evaluating the buyer's efficiency by the price variance, surely the difference between standard rate and actual rate at date of acquisition is the only measure. Any relevance of a future date is related not to a buying decision but to the borrowing or lending decision.

(b) If we are evaluating the foreign currency manager's efficiency there needs to be some analysis of the impact of exchange rate changes during the period of exposure. Surely during the exposure period the best measure of foreign exchange risk is based on the differences in spot rates. The effectiveness of the foreign exchange manager is at least to some extent measured by the extent to which forward contracts acquired have offset the exchange gain or loss; recognising as well the cost of such contracts. Financial Accounting Standards Board Statement No. 8, *Accounting for the Translation of Foreign Currency Transactions and Foreign Currency Financial Statements*, is used at least to imply some support for the authors' position. There is an incomplete citation from paragraph 212 of that publication. I repeat the whole paragraph, italicising the quotation included in the article: *An alternative approach for a forward contract intended to hedge a specific foreign currency transaction for the period between transaction date and settlement date is to use the rate in the forward contract rather than the spot rate at the transaction date to establish the related amounts payable or receivable.* Although a forward contract may limit or eliminate exposure on a payable or receivable denominated in foreign currency, the Board views such a forward contract as an independent transaction that should be accounted for separately. It also believes that the original discount or premium on a forward contract normally reflects an interest rate differential between two countries which should be recognized over the life of the contract if the contract hedges a foreign currency exposed net asset or net liability position. Further, since *specific identification of individual forward contracts with related unsettled foreign currency transactions may not be readily ascertainable*, the procedures specified by paragraph 23 are a more practical approach.

In my view the meaning of 212 is opposite to that implied by the quotation. In the words of the common man, FASB said: 'It isn't conceptually consistent to use forward rates and in mind'



circumstances you cannot do it anyway' (my words, not theirs).

One last comment is stimulated by the authors' failure in their review of pronouncements to include the exposure draft (E11) entitled *Accounting for Foreign Transactions and Translation of Foreign Financial Statements* published in December 1977 by the International Accounting Standards Committee. Their presumption could have been that E11 could contribute little to accounting thought on the matter since it did not choose one of the temporal or closing rates, nor require gains and losses in income, nor explicitly

deal with forward contracts. My view is to the contrary.

E11 required disclosure of gains and losses and leaned heavily in favor of use of the spot rate for recording transactions. While still somewhat ambiguous on use of the forward rate it certainly was clearer on this issue than the publication by the ASC in the United Kingdom. If financial statements worldwide were to comply with E11 the quality of financial reporting on this subject would be greatly improved even in such countries as Australia, Canada and the United Kingdom.

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# A Price Parity Theory of Translation: a Comment

John Flower

Professor Patz's article (Winter 1977) makes a number of interesting points relating to the preparation of consolidated accounts of multinational companies that involve the translation of accounts from one currency to another currency.

To my mind his two major points are:

- (i) that translation is essentially a mathematical operation that can in no way affect accounting principles;
- (ii) that in preparing the consolidated accounts of multinational companies, the overriding principle should be to measure the general success of overseas subsidiaries and not simply their ability to remit cash to the holding company.

Professor Patz justifies his position in these two matters by arguments with which I have a lot of sympathy, but with which in the end I find myself in basic disagreement.

## The translation process

Professor Patz defines translation as 'a mathematical process of transforming measurements taken in terms of one scale of measurement into a second scale of measurement'. He uses as an analogy the conversion of the measurement of a man's height taken in America in terms of yards into metres so that it can be compared with a height measured in France. In relation to this example he asserts: 'It was a purely mathematical exercise. There was no subjective judgement inherent in the process'. The conversion rate from yards to metres could be found by comparing a yardstick with a metrestick. By analogy he argues that the translation of currencies is a purely mechanical and neutral operation.

There is clearly an element of truth in this. Professor Patz is rightly very critical of accountants who take advantage of the translation process to introduce procedures that are essentially contrary to generally accepted accounting principles

(GAAP), e.g., by anticipating the occurrence of future events. I am sure that most accountants would accept his statement that 'any theory of translation should not take on the task of changing GAAP but, rather, should accept the accounting principles deemed sufficient at present as given.'

But in asserting that translation is a purely mathematical exercise, he is overstating his case. Perhaps he was led astray by his oversimplified example. Translation from dollars to francs is much more complex than conversion from yards to metres, for the fundamental reason that whereas the conversion rate from yards to metres remains invariant at 1.093:1, the exchange rate from dollars to francs fluctuates over time, in fact from day to day. The accountant faced with the task of translating an item from dollars to francs has at his disposal not a single conversion rate, but literally hundreds of rates. The process of deciding which rate to use, which is of course the fundamental problem in translation, is essentially a process involving skill and judgement, and can never be a purely mechanical process.

## The purpose of foreign operations

In relation to the second point, Professor Patz describes traditional translation theory in terms with which I am fully in accord: 'Traditional translation theory involves a concept of the [subsidiary] firm as an extension of the parent, a source of dollar cash flows'. 'The basic premise is that the objective of conducting foreign operations is to generate cash flows to the domestic parent'. Professor Patz queries this approach. He presumes a more fundamental objective for foreign operations: 'to maximise command over goods and services locally'. In effect the foreign subsidiary is treated as carrying on business largely for its own benefit.

Clearly Professor Patz has a point. The fundamental premise of traditional translation theory no longer appears relevant in those situations where the foreign operations of a multinational group are substantially more important than those undertaken in the home country, and where remittances to the holding company are small in relation to the magnitude of the trading operations. For example, it seems a little strange to assert that the Royal Dutch Oil Company carries on its world-wide business (90% of it outside the Netherlands) for the sole or principal aim of benefiting its Dutch shareholders.

But again Professor Patz overstates his case. Translation affects only the consolidated accounts which are prepared principally for the benefit of the shareholders of the holding company. It is quite reasonable to base these accounts on the premise of the foreign subsidiary being an extension of the holding company. The success of the subsidiary in meeting the aim that Professor Patz ascribes to it ('the maximisation of command over

goods and services locally') is measured by its own (local currency) accounts, which are not affected by translation.

### Conclusion

Finally, I would like to point out that, in my opinion, Professor Patz errs in implying that his analysis of the nature of the translation process (i.e., that it is purely mathematical) enables him to discover the basic premise on which the consolidated accounts of multinational companies should be based (i.e., the maximisation of command over goods and services locally). It is, I believe, self-evident that the principles on which these accounts should be based should be established with the aim of meeting the information needs of the parties involved, and that the translation process, which to quote Professor Patz is 'a mathematical exercise' which is 'neutral' and 'has no point of view', is completely subservient to these ends.

# A Price Parity Theory of Translation: a Reply

D. Patz

This reply takes its present rather odd and somewhat lengthy form for several reasons. The comments by Flower on my paper are quite general ones, principally asserting I overstated my case, but without a great deal of detailed argument to support this assertion. I make this observation not to criticise; it is difficult enough to present reasonably complete arguments regarding the quite complex issue of translation in the context of a full paper or even several papers. However, my difficulty in determining exactly where and why Flower disagrees with me is one reason this reply takes the form it does.

Another reason is the very complexity just mentioned. Even in the simplest translation situation one potentially confronts two measurement systems, two price levels, two rates of change in price level, two currencies, various exchange rates and rates of change therein, and is faced with the task of handling all these variables with some semblance of theoretical consistency. Discussion with brevity, given a present lack of extant literature to refer to (rather than produce), is correspondingly frustrated. Broad theory construction, to which the original paper is addressed, in one sense at least is like putting together a jigsaw puzzle having a large number of pieces, all of which cannot be used in any single attempt to construct a final picture, and where often the very same pieces can be put together in more than one way. Perhaps, in the present situation Flower has not considered all of the pieces. Alternatively, if I have overstated my case I have presumably done so by misinterpretation of the pieces, by overlooking a critical piece, or by forcing pieces together that in fact do not fit. In any event it seems necessary to give some additional consideration to the pieces of the puzzle, the sub-issues I considered and reached conclusions on in formulating the theory summarised in the original paper, before attempting to respond to comments dealing with 'final pictures'. Also, such an exercise may further aid Flower or others in specifically

identifying where I may have erred, and perhaps generally serve to reduce the overall issue to a set of manageable parts about which we can effectively argue.

Hence the present format. In Part I, I present a series of Chambers-like arguments which constitute a background to the conclusions I have reached in formulating the Price Parity Theory. They are presented to serve the ends just indicated, as well as to respond directly to Flower's comments with some degree of conciseness and brevity which is attempted in Part II. Of course the arguments presented are themselves incomplete, being unaccompanied by the research and reasoning underlying them. Likewise, it is not possible here to convincingly demonstrate that the Price Parity Theory is compatible with them, though it is believed this can be done.

## Part I

### Background arguments underlying the price parity theory of translation

#### *Foreign Operations*

- 1.1 Foreign operations of US/UK companies are material in the aggregate and at the firm level.
- 1.2 A significant number of US/UK companies have material interests in foreign operations so that the quality of US/UK reporting in general is affected by the quality of the results of translation methods now employed.
- 1.3 The general character of foreign operations has significantly changed from that which was extant when conventional translation methods were devised.
- 1.4 The characteristics which typify the contemporary multinational enterprise suggest that a general theory of translation adopt a separate entity, going concern view of foreign-based subsidiaries and affiliates; i.e., an

entity rather than proprietary accounting philosophy is appropriate.

- 1.5 Assumptions regarding wholesale remittance of earnings or conversion of net assets to a particular currency are inconsistent with a separate entity, going concern view of the foreign-based firm; a general theory of translation should not presume the occurrence or outcome of events or transactions without objective evidence comparable to that required in other areas of financial accounting.
- 1.6 The economic environments of countries are more or less homogeneous within each country but differ markedly so as to introduce a place significance to accounting measurements taken within the confines of a particular environment.
- 1.7 A primary objective of translation, while providing a common counter, is to adjust for or otherwise preserve the place significance of foreign accounts so as to effect translation with a minimum loss in the information content inherent in those accounts.
- 1.8 Consistent with the objective of preserving the place significance of accounts and the nature of the foreign firm as a separate entity, going concern, the measurement of foreign held resources (and changes therein) should be in terms of their present location.
- 1.9 Inflation is a significant environmental variable and rates and patterns of inflation vary significantly between countries.

#### *Uses and Users*

- 2.1 A general theory of translation must be consistent with the uses to which translated information is put.
- 2.2 The need to translate arises primarily from the need to provide a homogenous measurement unit for consolidation and the need to provide a single frame of reference to users of translated financial information.
- 2.3 Translation as a prelude to consolidation should be considered in a general theory of translation as a primary use of translation.
- 2.4 The nature of consolidation requires that translation result in balances comparable in form and content to the domestic balances with which they will be integrated.
- 2.5 Related to 2.4 above, and consistent with practical constraints and the nature of translation as a separate process, a general theory of translation must be compatible with the

accounting principles in the domestic country.

- 2.6 The output of the translation process serves as input for important managerial decisions.
- 2.7 Managerial decisions can be significantly affected by choice between the various exchange rates often available for use under current translation methods.
- 2.8 Hedging is a separate activity where the costs should be separately identified and set out in financial statements of multinational firms.
- 2.9 The rationality of many managerial decisions rests to a great extent upon the validity of the exchange gains and losses recognized in current translation accounting.
- 2.10 Foreign exchange rates and foreign price levels are important environmental variables relevant to the interpretation of translated accounting information.
- 2.11 In light of user needs, translation should involve timely recognition of the action of environmental variables having *real economic* consequences for the firm.
- 2.12 In light of user needs, translation should involve timely recognition of the *objectively identifiable and measurable* impact of environmental variables on the firm.

#### *Exchange Rates*

- 3.1 The general character of the international monetary system and the character of exchange rate behaviour has significantly changed from that which was extant when conventional translation methods were devised.
- 3.2 Observation of the exchange rate environment establishes that frequent, abrupt, and material change in exchange rates, both upward and downward relative to any currency, is a fact.
- 3.3 Basic assumptions with regard to exchange rate behaviour which are inherent in traditional methods of translation do not generally correspond with exchange rate behaviour as observed in the real world.
- 3.4 Observation of the exchange rate environment establishes that several exchange rates may exist for a single country at a single point in time and that these rates may differ materially.
- 3.5 If exchange rates are used, the choice of one available rate over another can materially affect the results of translation.
- 3.6 If exchange rates are used, the choice of 'the'

- appropriate rate is ambiguous and the alternatives and choices involved give rise to complex technical and theoretical problems which have no reasonable solutions.
- 3.7 If exchange rates are used, resolution of the problems associated with multiple exchange rates involves cost to the firm and gives rise to accounting problems of consistency and comparability.
  - 3.8 If exchange rates are used, one result is the capturing of temporary and reversing movements in rates, thus giving rise to translated values of dubious and potentially misleading character.
  - 3.9 A general theory of translation should be applicable to any and all conditions which may be experienced with the exchange rate environment; assumptions as to the permanence of particular exchange rate levels and the future behaviour of exchange rates are unwarranted.
  - 3.10 Any theory of translation should embody rigorous justification regarding the suitability of the translation rate to be used; the information content of the results of translation rests considerably on the meaning inherent in the rate chosen.
  - 3.11 All currently acceptable US/UK methods of translation use exchange rates exclusively for translation of financial statements.
  - 3.12 No rigorous defence for the use of exchange rates has been offered; the relevance of exchange rates to accounting valuation and income measurement has been assumed and has not been established.
  - 3.13 Levels of and changes in exchange rates are themselves a result of a complex interaction process involving numerous other environmental variables in complex mixes.
  - 3.14 Many of the economic, political, and other variables which enter into the determination of exchange rates are of questionable relevance to accounting valuation and income measurement.
  - 3.15 Specific identification of the variables involved in any particular exchange rate change is likely to be difficult, highly subjective, and perhaps impossible.
  - 3.16 Objective and separate identification and measurement of the real economic effect of an exchange rate change on firm values is highly unlikely: such identification and measurement requires dubious assumptions regarding the occurrences of future exchange transactions at designated rates and/or the occurrence of events such as price movements in the countries involved.
  - 3.17 Any assumption that exchange rates generally adjust in a reasonably precise, timely, and consistent fashion is empirically questionable.
  - 3.18 Any assumption that inflation and currency depreciation move together, except perhaps in the very long term, is empirically questionable; exchange rates do not appear to consistently reflect the relative value or purchasing power of currencies either at points in time or through time.
  - 3.19 The effect of a change or movement in an exchange rate on the economic position and results of operations of a foreign-based firm would appear generally indeterminant at the time such event occurs.
  - 3.20 Exchange rates are prices for specific commodities, i.e., currencies; as in other areas of accounting there is no *a priori* reason to apply this price to other than an actual or reasonably assured transaction involving the specific commodity at the specific price.
  - 3.21 Exchange rates, as market prices, do not appear to be especially relevant environmental variables for accounting valuation purposes, except where transactions at these prices have occurred or are expected with near-certainty to occur.
  - 3.22 Exchange rates, as approximations of relative value, do not appear *a priori* to be suitable surrogates for direct measurement of relative currency value and the effects of inflation on firm values.
- Contemporary Translation Practice*
- 4.1 An examination of the development of contemporary translation methods having authoritative support suggests that existing methods require broad re-examination.
  - 4.2 The current-noncurrent, monetary-non-monetary and all-current-rate methods differ as to the basis considered appropriate for distinguishing between those balance sheet accounts requiring translation at current exchange rates and those which require translation at historical rates.
  - 4.3 Definition of the account dichotomy to be used in applying current and historic translation rates is equivalent to definition of the unrealised gain or loss to be recognized and vice versa.
  - 4.4 The current-noncurrent method and the monetary-nonmonetary method differ pri-

- marily in their respective treatment of inventories and long-term receivables and payables.
- 4.5 Contemporary translation practice is typified by a wide range of translation methodology and a lack of uniformity in the application of that methodology.
  - 4.6 Choice of one method of translation and one approach to choosing and calculating appropriate exchange rates over another method and set of exchange rates can materially affect the results of translation and therefore the comparability and usefulness of financial statements which include foreign operations.
  - 4.7 An obvious need exists for greater uniformity in translation practice.
  - 4.8 A general theory of translation should provide a basis for unambiguous specification of the translation rates appropriate to the various situations encountered in translation.
- Contemporary Translation Theory*
- 5.1 Two major alternatives exist with regard to the central purpose to be adopted in translation; translation can seek to measure changes in the remittable domestic currency value of foreign held resources and the remittable domestic currency value of the results of operations, or it can serve to restate foreign accounts in equivalent domestic currency magnitudes.
  - 5.2 Contemporary translation methodology and the theoretical reasoning which exists to support this methodology constitutes tacit acceptance of translation as a separate measurement process with varying interpretation as to how best to effect such measurement.
  - 5.3 By necessity translation as measurement requires assumptions regarding the occurrence of future events in order for the measurements taken to be meaningful.
  - 5.4 No particular set of assumptions regarding the occurrence of future events and therefore specification of the real effects of an exchange rate change is superior to another on an *a priori* basis.
  - 5.5 The current-noncurrent, the monetary-non-monetary and the all-current-rate methods differ conceptually as to perception of the probability that a potential gain or loss created by a change in exchange rates on any particular resource will be realised.
  - 5.6 The meaningfulness of the results of any method of translation which is measurement is a function of the extent to which the assumptions regarding future events inherent in the method hold in any single instance of translation.
  - 5.7 Observations on the nature of contemporary foreign operations and the behaviour and information content of exchange rates indicate that the assumptions inherent in traditional translation are seldom true to the extent necessary to produce meaningful results.
  - 5.8 Diversity in translation practice, as in translation theory, may be related to the wide range of assumptions as to future transactions and events possible in the complex context of real-world operations.
  - 5.9 At present there appears to be general acknowledgement that contemporary translation methods do indeed often give recognition to non-existent exchange gains and losses.
  - 5.10 The usefulness of the information which traditional methods aim at providing is undocumented and its relevance is highly suspect.
  - 5.11 The current-noncurrent method, the monetary-nonmonetary and the all-current-rate method cannot be expected to produce logical results on a consistent basis; indeed, it is highly questionable whether any translation-measurement method applied to statements prepared under generally accepted accounting principles can produce results subject to rational interpretation.
  - 5.12 The approach advanced in *ARS No. 12* is of the same general class of methods describable as translation-measurement; as such, the approach merely retains and perpetuates most if not all of the undesirable characteristics and the problems associated with present translation practice.
  - 5.13 A general theory of translation must reject measurement of the remittable domestic currency value of foreign-held resources as a guiding purpose for translation.
  - 5.14 A general theory of translation should be built around the central purpose of restatement, in domestic currency terms, of the quantitative information inherent in foreign accounts prepared under generally accepted accounting principles.
  - 5.15 Historical cost is an economic magnitude which attains significance relative to the time and place of its measurement.

- 5.16 Preservation of historical cost in the conventional sense of retaining the original number assigned to an historically-costed asset is meaningless in the multidimensional context of translation.
- 5.17 Preservation of historical cost in the conventional sense is accomplished by following generally accepted accounting principles in the preparation of individual sets of financial statements.
- 5.18 Preservation of historical cost in the multidimensional context of translation-consolidation involves preservation of the meaning of historical cost as an expression of economic sacrifice, which in turn involves preservation of historical value relationships.
- 5.19 There is *a priori* appeal in the proposition that accountants measure particular attributes of assets and liabilities and that such measures have temporal characteristics.
- 5.20 A general theory of translation should incorporate consideration of the attributes measured in current accounting and the temporal characteristics of such measurements when the question of preserving the information content of foreign-sourced accounting measurements arises.
- 5.21 Accounting principles should not be confused with the attributes measured in applying those principles, nor should principles and attributes be confused with the meaning associated with account balances.
- 5.22 Theoretical arguments regarding the traditional approach to translating capital, revenue and expense items are superficial and unconvincing.

## Part II

### Reply to specific comments

1. 'Translation from dollars to francs is much more complex than conversion from yards to metres ... [because] ... whereas the conversion from yards to metres remains invariant at 1.093:1, the exchange rate from dollars to francs fluctuates over time, in fact from day to day.'

The price of nearly all commodities, not just that for foreign currency, varies from day to day. Hence, the above argument is equivalent to asserting that measurement of historical cost is made complex by the fact that prices change for

assets. Yet I would think that Flower and I would agree to use the price at date of acquisition to *historically cost* any given assets. I believe we would both conduct thereafter the quite mechanical, mathematical and neutral process of multiplying units of assets by their historical acquisition prices, and not feel particularly negligent in doing so.

Yet we would both have at our disposal 'literally hundreds of rates' (prices), and still not have to exercise substantial 'skill and judgement'.

The *process of applying* price parity translation is indeed less complex than conventional methods, but this is quite unrelated to varying rates (exchange rates or price parity indices), but is because the underlying theory allows unambiguous identification of the rates to use consistent with the theory underlying their use. If one is required to exercise truly substantial skill and judgement on a case by case basis then I submit either something is wrong with the underlying theoretical system and basic principles followed, or concrete and convincing arguments must be made to justify the need for ambiguity. In his comments, Flower presents no such arguments. In the original paper I present several arguments in favour of the proposed approach. Above are several arguments against conventional approaches. See 3.20, 3.21, 3.1-3.22, 4.1-4.8, 5.1-5.22.

2. 'But again Professor Patz overstates his case... It is quite reasonable to base these [consolidated] accounts on the premise of the foreign subsidiary being an extension of the holding company.'

Here I can only ask in return—*why* is it 'quite reasonable'? Flower appears to admit I have a point: 'the fundamental premise of traditional translation theory no longer appears relevant in ... [some situations].' If the reason for his objections is that he sees these situations inadequate in number to serve as a basis for general principles, then he has an *a priori* view of the distribution of firms with material interests in foreign operations significantly different from my own.

My own perception of the extant distribution of firms wherein translated results are a material factor is captured in Figure 1. The x axis in Figure 1 constitutes an attribute continuum beginning at the origin with firms whose foreign operations are fundamentally import-export activities through those characterisable as national operations with international business operations



to extreme definitional forms of the international or multinational firm as characterised by such writers as Mueller and Scott. In turn, this continuum is itself some weighted combination of several individual characteristic-continuums. The degree to which any particular firm possesses each presumed pertinent characteristic, in combination with the degree possessed of each other characteristic, defines that firm's position along the weighted classificational-continuum (x axis). These individual characteristics would likely at least include remittance-reinvestment behaviour, the permanence, continuity and normalcy of foreign operations conducted, management view of the foreign elements of the total system, absolute and relative magnitude of foreign operations conducted, direction (foreign-domestic) of operational expansion or emphasis, goals adopted by management regarding foreign operations, and the degree of centralisation and worldwide co-ordination. All of these (and perhaps other characteristics) are unlikely to be accorded equal significance from the standpoint of an accounting concept of the firm upon which to base translation practice and theory, hence the concept of weighting in the x axis. For example, remittance-reinvestment behaviour, or permanence and continuity of foreign operations would probably be accorded greater weight than say degree of operational centralisation.

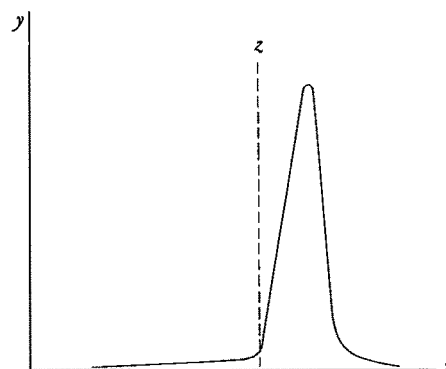
The point is, the only source of overstatement of my case that I can discern at the moment is the general invalidity of Figure 1 as a description of reality. That is, Flower could possibly assert a flattish, near-uniform distribution in place of that in Figure 1 and therefore effectively assert that no general theory and set of general principles is possible. This would certainly be consistent with an argument that material skill and judgement is required on a case by case basis. Alternatively, he could posit a similar distribution to that shown in Figure 1, that is a peaked one (indicating a general theory and general principles are possible) but one skewed in the opposite direction. This would be consistent with arguing it is 'reasonable' to base general theory and principles on traditional remittance-value grounds. However, I believe on available evidence that Flower would have considerable difficulty defending either of these two alternative distributions though I would be first to admit this remains a basically unanswered empirical question (see Patz, *Journal of Business Finance and Accounting*, Autumn, 1977). Of course, he may have quite different reasons for disagreeing with me, but

none are present in his Comment. See 1-3, 1-4, 1-5, 3-1-3-22, 4-1-4-8, 5-1-5-22.

3. 'The success of the subsidiary in meeting the aim that Professor Patz ascribes to it ('the maximisation of command over goods and services locally') is measured by its own (local currency) accounts, which are not affected by translation.'

I agree with this point, to the extent that GAAP permit such measurement. However, I fail to appreciate its significance in the context in which it is raised. We appear to agree on the significance of the shareholder user group, though I would expand this to 'capital market agents'. Yet typically the local currency accounts would not be available to this user group. If they were, it is unlikely that this group would be familiar with the individual cost-price structures of the economies involved. More important, it seems highly improbable that this group could assimilate say the individual reports of 50 subsidiaries operating in 20 different countries and incorporate this information in their respective decision models directed at assessing the risk and future

**Figure 1**  
**Theoretical Distribution of Multinational Firms**



y—Number of multinational firms, as defined simply as those domestic firms having material interests in operations conducted abroad.

x—A classificational continuum by nature of the firm from export-import, through national firms with foreign operations to international or 'multinational' in the sense used by Mueller and Scott; a conceptual ordering of the total population of multinational firms based on the characteristics they exhibit.

z—The implied dichotomisation, the right of which constitutes the 'general case.'

return distribution associated with holding a share in the *parent* company.

So we return to the consolidated accounts of the group which does, and reasonably so, make up part of the information set available to investors and are right back to the question of *how* to translate the foreign element of these accounts and *why*. I have argued for restatement of the information content (which we appear to agree exists) residing in the local currency accounts, as opposed to trying to measure a remittable domestic currency attribute of foreign assets as if they merely constituted (individually) investments with individually identifiable and measurable risk and expected domestic currency return characteristics. My reasons reside in the original paper and the arguments presented above. See 1·6, 1·7, 1·8, 1·9, 2·2, 2·3, 2·10, 5·1, 5·2, 5·13, 5·14, 5·20, 5·21.

4. 'Finally..., in my opinion, Professor Patz errs in implying that his analysis of the nature of the translation process (i.e., that it is purely mathematical) enables him to discover the basic premise on which the consolidated accounts ... should be based.'

First, it is my analysis of the problem, not the process, which leads me to suggest an alternative basic premise upon which to build a translation theory and consistent translation methodology. In prescriptive theory construction only the problem, not any particular process, is given.

Second, all *processes* of translation of which I am aware are mathematical or mechanical. What is not mathematical or mechanical is the choice between alternative translation information production systems, between basic theoretical approaches and basic principles. Third, while it is self evident, it is equally empty merely to assert that the underlying principles 'should be established with the aim of meeting the information needs of the parties involved'. Once again, I can only respond by saying the original paper and the argument summarised above give at least some partial consideration to the information needs issue, and some explicit counter-arguments or alternative points of view are required before my 'case' may be evaluated. See 1·1, 1·2, 2·1-2·12, 3·5, 3·7, 3·8, 3·14, 3·16, 3·19, 4·6, 4·7, 5·4, 5·6, 5·7, 5·9, 5·10, 5·11.

# Patz on Parities, Exchange Rates and Translation

F. L. Clarke

The argument for using purchasing power parities rather than official exchange rates in the translation of foreign currency account balances was taken up in this journal recently by Professor Patz (*Accounting and Business Research*, Winter 1977, pp. 14–24). His promotion of parities and criticism of exchange rates relied somewhat on claims that (a) using parities is neutral regarding the future action by managements, whereas using exchange rates is biased towards measuring changes in 'future remittable dollar values' (p. 22), (b) exchange rates *per se* ignore what he labels the 'place dimension' of monetary units (p. 20) and, (c) the translation mechanism is independent of valuation systems and, therefore, can be derived without reference to them.

If those claims could be sustained the translation issue would truly have been chopped down to size, leaving only the administrative frills that usually surround standard setting to be added. On the other hand the 'parities versus exchange rates' debate<sup>1</sup> might properly be put aside as a non-event if Patz's claims do not stand up. In this paper it is argued that they do not.

## Whither 'neutrality'?

We have no quarrel with the notion that translation procedures ought to be neutral. It is Patz's use of neutrality as the criterion ruling against the use of exchange rates and in favour of parities to which we object, on two grounds. Firstly, the reasoning from which neutrality emerges as a criterion draws upon doubtful distinctions between the processes of measurement and translation. The argument ends up a mere contrivance apparently designed to allow the introduction of a notion of a 'place dimension' in translation.

Secondly, rather than establish bias when exchange rates are used, the opposite is strongly implied—that, in fact, using parities in translation is even less neutral than the use of the official rates is said to be.

The contrasting of measurement and translation (pp. 15–16) immediately follows Patz's example in which individuals' heights are measured—some in metres and the others in yards, depending upon whether they resided in Europe or in the USA. The essential point to the exercise was that in different places different scales are often used to measure an invariant property. And that each measure is easily translated into its equivalent in the other scale with the use of a conversion ratio. A number of the conclusions drawn from the example are *non sequiturs*.

Measurement we are told '...involves mathematical operations' and translation '...is a mathematical operation' (p. 15, emphasis added). No explanation is given for the fine distinction. It is all the more curious when later translation is identified as a 'metrics problem'<sup>2</sup> (pp. 16, 17, 22, 23). How it can be a measurement-like problem yet have different basic characteristics to measurement is anything but clear. We might even argue convincingly that, contrary to that distinction, in some circumstances, translation or restatement from one scale into another is properly part of the measurement process. Suppose, for example, a situation in which the length of an object is required to be stated in yards, but only a metre stick is on hand. If the stick is laid along the length of the object, the number of metre units noted and the 1.093:1 ratio applied to calculate the yards equivalence, at what point

<sup>1</sup>See for example F. L. Clarke, 'A Note on Exchange Rates, Purchasing Power Parities and Translation Procedures', *Abacus*, June 1977, pp. 60–66.

<sup>2</sup>We take it that this term is used in its usual sense to denote 'measurement problem', as in *Chambers Twentieth Century Dictionary*, 'metrics: the theory of measurement'; and the *Shorter Oxford Dictionary*, 'metrical: the science which deals with the comparison and relations of spatial magnitudes'.

did the measuring process cease? Patz would say it did so once the metre units were summed, I suspect. Why? Is the restatement by 1.093:1 really any different in nature *as a process* to adding up the number of times the metre unit was laid out? Each it seems may reasonably be regarded as intermediate calculations in *one* process moving towards a final numerical statement in the desired scale. The cut-off point at which measuring is deemed to cease is arbitrary and of no practical significance. It certainly does not help to illustrate any unique characteristic of translation.

Patz further claims that measurement '... defines its own circumstances... creates new relationships... can involve a point of view' (p. 15). But, whatever relationship existed between the properties of objects did so whether measuring is undertaken or not. The relationship between the heights of Patz's men in Europe and the USA in no way depended upon actual quantification of any kind. Nor did the relationships between each man's height and the respective metre or yard sticks. The process of lining the men up against each other or each of them up against either the metre or yard stick did not create anything new at all. Curiously, reference is made in the course of that claim to Sterling and Chambers on the nature of measurement and by inference therefore to the work of Campbell<sup>3</sup> and Stevens<sup>4</sup> on the subject. The essence of those descriptions of measurement processes is that they are objective, not subjective and creative as Patz represents them. Neither Chambers nor Sterling disturb that.

However, persistent reference to measurement is important to his argument for using parities. It gives the opening for him to draw on the height example to launch the idea of a *place significance*, used later to distinguish translating with parities from translating with official exchange rates. Measurements in metres in Europe and in yards in the USA are said to expose a place significance characteristic of the measures. In fact, they do no such thing. At best they demonstrate a bias in favour of particular scales in different places. For if measurements in different places each had a unique place significance we would expect that measuring height in Europe, for example, could only be effected in metres, and in the USA only in yards. Otherwise the characteristic would not

be unique. But we can measure height and length in metres or yards wherever and whenever we like. We can, if we choose, do so in both scales at the same time. Preference for using one scale or the other is more a matter of custom and convenience<sup>5</sup> than an imperative imposed by physical laws. When *place significance* is recalled later in the parity case it is entirely different in nature to the description of it in the height example. It seems that the example was good only to introduce the *label*, not the actual notion he wants to make use of.

The measurement/translation contrast does not give any strength to the claim that translation is neutral. Nor does it really indicate in what ways translation is neutral when other processes (measurement, for example) are not. It does not expose a unique place significance of such processes. It seems that all it does is promote a new 'label' in the literature.

Despite exhortation for neutrality when exchange rates are discussed Patz abandons it when extolling the virtues of using parities in translation.

'It could be argued convincingly' he says (p. 20) that traditional translation is intended to measure the 'future remittable dollar value' of foreign held resources. Nevertheless he does not do so. And it is doubtful if any such argument is sustainable, as every statement of the kind  $\text{£}X = \text{\$}Y$  is merely one of the *equivalence in exchange* of one currency for the other. It is a statement of a price, no different in character to the statement of the selling price of one's home. No valid inference that an actual exchange (conversion and remittance) will occur or even is contemplated, can be drawn from any such statement. It is completely neutral.

The propositions that translation ought to be '... directed toward emphasis on the utility of foreign-held resources to the *foreign company*' (emphasis added), and that there ought to be expectations of those resources continuing to be held and used in the foreign economies (p. 22) however, are not neutral. If using parities rests upon such propositions (Patz considers it does, pp. 22-23) and rules against the equivalence exchange rates denoted, we are forced to conclude it is less neutral than using the official rates.

<sup>3</sup>N. R. Campbell, *What is Science?* (New York: Dover Publications, Inc., 1962), p. 111.

<sup>4</sup>S. S. Stevens, 'On the Theory of Scales of Measurement', *Science*, no. 103, 1946, pp. 677-80.

<sup>5</sup>Of course legal specifications governing rules of reporting or advertising measures may compel the use of a particular scale within national boundaries. But they do not prevent the use of any scale in the measurement process provided the appropriate conversion is effected.

### Misplaced 'place' significance

That domestic and foreign monies have a unique characteristic insofar as each is exchangeable for goods and services only within their respective national boundaries is no more than a truism. It is the kind of place significance that Patz tried to illustrate with his example of measuring the height of men in Europe and the USA. But it fell down there as an analogy to the foreign currencies case, for whereas the height scales are usable in every location, it is generally neither customary nor legal to trade with foreign money outside its relevant national boundary. Domestic and foreign money are not alternatives as a medium of exchange within (say) the domestic national boundaries in the same way that yard and metre sticks are for measuring height. They do, therefore, have a unique characteristic. But it is purely a function of location unalterable by quantitative adjustments.

A 'place significance' in relation to the respective purchasing powers of domestic and foreign monies is a fiction. It implies a comparison between the exchangeability of domestic money for domestic goods and of foreign money for foreign goods as if all but the rate of exchangeability in each case were alike. We have already accepted the idea that domestic money and foreign money are not homogeneous because of their respective place locations. Patz accepts that too (p. 19). It is no less true that domestic goods and foreign goods are heterogeneous because of their location, even if they are identical regarding their physical and technical attributes.

Goods and services  $a, b, c, d, \dots n$ , in the domestic economy for example, are not the same as goods and services  $a, b, c, d, \dots n$  in a foreign economy. Neither bundle nor any of their components are immediately available for consumption outside their respective economies. Nor is either substitutable for the other as it stands. Substitution would require at least a further production input in the form of transportation from one economy to the other.<sup>6</sup>

It is not meaningful to compare the purchasing powers of domestic and foreign monies. Comparisons of that ilk are encouraged by interregional comparisons of purchasing powers of domestic money prevalent in the price level debate<sup>7</sup> and

by Patz's like statement that '...the dollar in Austin, Texas, commands an appreciably larger quantity of goods and services than a dollar in New York City... The measure [of purchasing power] possesses a place significance' (p. 19).

Good  $a$  in Austin is not the same as good  $a$  in New York City. It is not merely a locational difference which transportation could rectify, but a whole set of circumstances (social, political, environmental and legal) surrounding the possession of good  $a$  in New York City as opposed to it being in Austin or elsewhere. A dollar spent in New York City is not subject to the same experiences as a dollar spent in Austin or as its sterling equivalent spent in Manchester. Though, it is not so much the purchasing power of each which we can meaningfully say is different (and has a place significance) as the goods and services for which the money in each case is exchangeable.

Non-comparability in the structures of national prices is a further barrier to the interspatial comparison of purchasing powers. Bundles of goods and services for which domestic money is exchangeable in each economy include those currencies foreign to it. The structure of prices in the UK includes the sterling price of US dollars and of every other currency traded in the market. In the USA the structure of prices includes the dollar price of sterling and the other currencies. And changes in national price structures include the effects of changes in the absolute price of each other foreign currency. In that respect domestic and foreign currencies are always heterogeneous on other than a purely locational criterion, for in the international setting they will for ever face different goods and services and be part of different price structures.

Rating the price level of one country against that of another in the manner of calculating purchasing power parities has doubtful propriety, both as a mechanism for setting an equilibrium exchange rate and as an arithmetical procedure.

As a rate setting mechanism it ignores the role invariably played by governments in manipulating exchange rates for their domestic money, to achieve desired trading goals. Governments are constantly interfering in that way to channel the course of international trade. It ignores the differences in the composition of goods and services traded within each economy and those traded internationally. The purchasing power parity theory has been under attack constantly in the economics literature on that score.<sup>8</sup> It ignores the trans-

<sup>6</sup>See L. Von Mises, *The Theory of Money and Credit* (London: Jonathan Cape, English translation 1938), chs. III and IV for a discussion of these issues.

<sup>7</sup>See for example R. S. Gynther, 'Why Use General Purchasing Power?' *Accounting and Business Research*, Spring 1974, pp. 141-54.

<sup>8</sup>See Clarke, *ibid.*

portation costs of internationally traded goods. That is, it takes no account of the very place location dimension that Patz made so much of in his case for using parities in translation. And, of course, such rating makes no allowance for the differences in the national structures of prices of trading economies that we have just discussed above. The consequence of those obvious defects in the parity theory has been quite definite. Contrary to Patz's use of parities to denote the relative purchasing powers between trading economies, the economics literature seems to have settled quite firmly on purchasing power parities being, at best, indicators of the direction in which official exchange rates should change and perhaps of the rate at which they should do it. So it is not so much a case of exchange rates being surrogates for parities (p. 24) as parities being possible surrogates for exchange rates.

As an arithmetical procedure, rating index numbers denoting the price level in different economies involves an element of circularity. If we, for example, consider the variant price structures in separate economies, then the numerator and denominator of any such parity ratio are representative of different things.

Patz's notation demonstrates as much. His translation mechanism (p. 20) is of the form  $a_{12k}(X_{2z}) \times l_{2k}(X_{2z})/l_{1k}(X_{1z})$ . The  $X_{2z}$  and  $X_{1z}$  in the second part of the notation represent the separate commands over goods and services in the trading economies. The notation had to be different because the purchasing power differed: not just quantitatively as the notation implies but qualitatively as well. The numerical 'solution' that the notation implies ignores all of the qualitative differences just noted in the previous paragraph.

We could denounce such a ratio on the grounds of its impropriety at that point, for the numerator and denominator are heterogeneous and could not give an interpretable product. There is also an element of incest in the ratio, for the price structures to which the numerator and denominator are respectively related each include the price of foreign monies; each therefore includes part of the other. About the only plausible defence against the impropriety is the common belief that exchange rates 'wash out' those differences in the nature and composition of goods and services within the economies of trading nations. But to plead it is circuitous. It assumes and rests upon the prior existence of the exchange rate that the mechanism is intended to be setting. If we ignore the circularity and accept the defence, then the only conclusion we can draw

is that the exchange rate was properly set independently of calculating purchasing power parities. Whichever way we choose to take it, the parity mechanism does not do what is claimed of it.

### Measurement systems do matter

The translation debate has proceeded almost without questioning the assumption that it could be settled without concern over the basis of the measurement of the monetary properties of assets, equities, revenues and expenses. Patz's extension of translation's neutrality to exclude it from the influence of '... alternative measurement systems and the like' (p. 16) comes as no surprise, as it is consistent with most of the literature up to this time. His reference to Lorensen at this point is a ready source of evidence of the entrenchment of the assumption.

Yet, whether we use official exchange rates or purchasing power parities as a surrogate for them, the technical setting of translation always places a constraint on the circumstances in which it may be effected with complete technical propriety.

Translation is a monetary calculation in which the technical features of monetary calculation ought to be observed if its product is to be useful. Exchange rates are money prices of one currency for another. Parities are intended to be expressions of the relative purchasing powers of different currencies. Each, therefore, refers to a specific relationship between a given sum of dated money (in whatever currency) and specified bundles of goods and services existing at that time. In the case of exchange rates the relationship is part of what is described in conventional monetary economics as the *equation of exchange*; and the relative equations of exchange in the instance of purchasing power parities. As such, the canons of monetary calculation have to be observed.

Exchange rates (and their surrogates) are always contemporary prices for the other currency. Each exchange rate is dated and relates to money and the structure of prices faced by it at that time and no other. Translation with contemporary rates, then, dictates that contemporary *monetary measures* of the worth of assets and equities, and of revenues and expenses, are a necessary condition for technically effective translations. The use of current rates to translate past measures or past rates to translate current measures is technically wrong and, except by pure chance, not likely to produce a meaningful or useful result. They cut across the canons of arithmeti-

cal calculations in general and of monetary calculation in particular. Patz's references to Chambers and Sterling on the homogeneity problem (p. 17) support this line of argument.

The heterogeneity problem in the context of the translation of foreign currency amounts is not limited to the consideration of the appropriate exchange rate. We emphasised *monetary measures* in the previous paragraph since exchange rates as *contemporary prices* are relatable only to other contemporary prices, actual existing money sums or their equivalent. As prices, exchange rates are not relatable to many of the numbers that either appear in financial statements or which are being recommended to appear. They are not relatable, for example, to the products of present (discounted) value calculations for assets or liabilities and whatever other numbers are derived from them; or to the assumed future replacement costs of assets or services, depreciation calculations based on estimated future replacement prices, historical costs restated with price index numbers, the product of a Hyde type gearing adjustment and the cost of sales adjustments based on present, average for the period or anticipated future replacement prices.<sup>9</sup> Nor are they relatable to aggregates for assets and equities calculated

under the varying bases permissible within the generic title of 'value to the owner' in the manner of Sandilands' current cost accounting.<sup>10</sup> For none of those amounts is actual money existing at the time of translation, or even 'near money' or its equivalent. Translation using exchange rates is impossible with such data.

## Conclusions

Purchasing power parities have gained some popularity in the accounting literature without much evident regard for either the nature of exchange rates or the technical specifications of translation. Parities are not alternatives to exchange rates. At best, they are surrogates for them. Exchange rates are prices. Whether they equate with parities is of no consequence insofar as translation is the matter of concern. As prices in the economy where the translation takes place, exchange rates do not contravene any place dimension. In such a setting Patz's purchasing power place dimension is a non-event. Finally, effective translation rather than being neutral regarding measurement bases is dependent upon the nature of them.

<sup>9</sup>See *Inflation accounting—an interim recommendation by the Accounting Standards Committee*, November 1977, pp. 15–21.

<sup>10</sup>*Inflation Accounting: Report of the Inflation Accounting Committee*, HMSO, Cmnd. 6225 pp. 56–61.

# Replacement Cost and Owner Wealth

G. Edward Philips

In a recent exchange with K. P. Gee and K. V. Peasnell,<sup>1</sup> Boris Popoff<sup>2</sup> defends the idea that in certain circumstances net realisable value (NRV) is an exception to the widely accepted notion that replacement cost (RC) is an upper limit to the economic value of an asset to its owner. The purpose of this note is to suggest a resolution of the dispute and to outline some fundamental issues that need to be given more attention by accounting researchers.

Popoff's exception involves the case where  $NRV > RC > PV$  (present value). In this circumstance value to the owner is NRV in Popoff's view but RC according to Gee and Peasnell. The debate might have been more fruitful had it been noted that not only is RC in every case an upper limit to value to the owner, but with equal validity it can be shown that NRV is always a lower limit.<sup>3</sup> If both these limits hold, then it must be impossible for NRV to exceed RC because a valid lower limit can hardly be greater than a valid upper limit.

That both limits are valid may be supported as follows: NRV, the money amount that can be received by disposing of an asset, is always a lower limit to the amount by which the wealth of the owner is enhanced by owning the asset.<sup>4</sup> To include an asset at an amount less than NRV would be equivalent to disregarding some of the cash on hand in arriving at total assets since the owner of the asset has the power, by definition, to realise that amount of cash.

That RC is an upper limit to the value of an asset to the owner is usually explained negatively

—RC is the maximum loss that would result from disappearance of the asset. It is a maximum loss because, by definition, the asset can be replaced by the expenditure of RC. A more positive statement is that RC is an upper limit to the amount by which the wealth of an owner is enhanced by owning the asset. This can be defended as follows: if an asset enhances wealth by a given amount, N, then sale of the asset at any smaller amount will leave the owner worse off. But if N is greater than RC, sale of the asset at a price between N and RC will leave the owner better off, since he can pocket the excess of the sale price over RC and otherwise be in the same position as before. Accordingly, N, the enhancement of wealth from owning an asset, cannot be greater than RC.

If one accepts, as does the present writer, the validity both of RC as an upper limit and NRV as a lower limit, it follows that there must be something wrong with our definitions or measures of RC and NRV when NRV appears to be greater than RC, for a lower limit greater than an upper limit is logically impossible.

A number of things might cause NRV to appear to be greater than RC.<sup>5</sup> We might be

<sup>4</sup>This requires, of course, that wealth is measured by summing the money amounts of assets and liabilities. This is 'the case in present accounting practice and is true of all concepts of 'asset valuation'. Individual asset values become irrelevant when wealth is measured as the present value of future cash flows of the entity or as the 'value of the entity'.

<sup>5</sup>NRV cannot *actually* be greater than RC in a competitive market because the actions of buyers and sellers would promptly destroy the difference by taking advantage of it. If we assume the entity whose wealth we are measuring is a monopolist or monopsonist, it hardly seems appropriate to attribute the value of the monopoly power to specific assets which happen to be held at a point in time.

We tend to think that retail businesses are in a position where NRV is greater than RC because retail prices exceed wholesale prices. But NRV is not actually greater than RC in this case. Wholesale prices, adjusted upward for transportation and handling costs and the like, ordinarily are a good measure of RC, but retail prices usually are much higher than NRV. In order to sell his inventory a retailer must either wait a considerable time (with considerable expenses) or must reduce his selling prices. If we sum the price tags on a retail inventory as a starting point for a measure of NRV, we must adjust this sum downward for the costs of advertising, sales clerks, rent, interest and so forth and if we find the adjusted sum significantly exceeds replacement cost we can be sure we have made an error.

<sup>1</sup>K. P. Gee and K. V. Peasnell, 'A Comment on Replacement Cost as the Upper Limit of Value to the Owner', *Accounting and Business Research*, Autumn 1977, p. 312.

<sup>2</sup>Boris Popoff, 'Replacement Cost as the Upper Limit of Value to the Owner', *Accounting and Business Research*, Autumn 1977, p. 311.

<sup>3</sup>These limits were suggested in G. Edward Philips, 'The Revolution in Accounting Theory', *The Accounting Review*, October 1963, p. 707 and were discussed by David Solomons, 'Economic and Accounting Concepts of Cost and Value', in *Modern Accounting Theory*, ed. Morton Backer (Prentice-Hall, Inc., Englewood Cliffs, N.J., 1966), pp. 124-125. Solomons suggests the possibility that NRV may be greater than RC but does not note that this yields a lower limit which is higher than an upper limit.



measuring prices for different assets, at different times, in different markets, or in different groupings. A thorough theoretical analysis and empirical exploration of these would be valuable if accounting practice is to continue moving in the direction of current value.

RC and NRV are both concepts of a price in a market. In some active markets, they are readily measurable and it is easy to see that RC exceeds NRV. At a given time the purchaser of a security on an exchange will have an outlay greater than the proceeds to the seller, the difference representing trading costs plus possible taxes, etc. For any asset in a market at a point in time the market price to the buyer (RC) will always exceed the market price to the seller (NRV) by at least the amount of trading costs.

If we wish to measure RC and NRV of an existing asset, we will accordingly always find  $NRV < RC$  of an 'identical' asset. It is possible, however, that  $NRV > RC$  of an 'equivalent' asset. An example is a case where a machine with a smaller capacity (and lower price) is adequate to the needs of an owner of a large machine. In this instance the value that correctly measures the amount by which the owned machine enhances the wealth of the owner is NRV. This is not an exception to RC as a maximum, since RC of the owned machine is greater than NRV. Nevertheless, RC of an equivalent is a maximum to wealth enhancement whenever NRV of the owned asset does not exceed it.

Much research is needed into the definition and implementation of the concept 'equivalent'. More research is also needed into 'timing' and 'market'. Under what circumstances is it appropriate to measure prices that would result from rapid liquidation rather than 'normal' or 'optimum' timing? Is used equipment bought and sold in a different market from new equipment? To what extent should we consider markets in foreign countries? The list of relevant questions is a long one.

Another complication is what may be called the 'grouping' problem. It is commonly the case that the market price of a group of assets will not equal the sum of the prices of the assets in

the group. In using RC as a maximum and NRV as a minimum we need assurance that we are not misled to 'improper' grouping. This also requires further research.

The number of possible combinations of RC, NRV, and PV is reduced by the limitation that  $RC > NRV$  and also by  $PV \geq NRV$ . PV could take on an infinite number of values reflecting different holding periods, uses, etc. If PV is defined as the maximum present worth of all the possible alternative future sales prices, cost savings, etc., it cannot have a present worth less than NRV, which is one of the alternatives.<sup>6</sup>

As a result of these limitations there are only two possible positions of RC, NRV, and PV:

- (1)  $PV > \underline{RC} > NRV$
- (2)  $RC > \underline{PV} > NRV$

These inequalities disregard the possibility that two adjacent numbers may be equal, since this would not affect the conclusions reached. The concept which measures wealth enhancement to the owner has been underlined. When we introduce a replacement price of an equivalent (RCE) which is lower than RC, five additional possibilities arise:

- (3)  $PV > RC > \underline{NRV} > RCE$
- (4)  $PV > RC > \underline{RCE} > NRV$
- (5)  $RC > PV > \underline{NRV} > RCE$
- (6)  $RC > PV > \underline{RCE} > NRV$
- (7)  $RC > RCE > \underline{PV} > NRV$

Popoff's 'exception', then, is not an exception to the rule that RC is a maximum value to the owner, it is rather a reflection of the rule that NRV is a minimum value to the owner. NRV is the correct measure of the enhancement of wealth from owning an asset whenever NRV exceeds RCE or equals PV; in other words, whenever wealth would not be reduced by disposing of the asset. If an entity is in position (3) or (5), it should sell and replace with the equivalent which will put it in position (1). If the entity is in position (2) or (7) and  $PV = NRV$ , its wealth would not change by selling the asset. Sale would leave the entity worse off if in (1), (4) or (6), and also in (2) or (7) when  $PV > NRV$ .

Replacement cost is the correct measure of wealth enhancement only when the asset is in position (1). This, however, is the normal position—it is certainly the position the owner believes himself to be in immediately after acquiring the asset. If position (1) does not apply, the acquisition reduced wealth. Position (1) corresponds with the widely accepted notion that historical

<sup>6</sup>This definition of PV requires reaching beyond existing markets and calculating a discounted present value of expected future flows. The present writer has argued elsewhere that it would be desirable to develop a contemporary market concept that is not dependent, in principle, on any person's forecast of the future. An attempt to do this is in G. Edward Philips, 'An Entity-Value for Assets and Equities', *Abacus*, December, 1968, pp. 142-152.

cost properly measures current value at the time of acquisition.

If most assets are in position (1) most of the time, RC should be used most of the time in

accounting for wealth. To the extent that cases of other positions can be observed in which there are material differences between RC and competing values, RC must give way to other values.

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# Replacement Cost as Upper Limit of Value — Further Fallacies

E. R. Kermode

The comment on p. 312 of the Autumn 1977 issue of *Accounting and Business Research* does not deal with faults in Popoff's note (p. 311) which are apparent to a practising accountant.

The argument starts with  $NRV > RC > PV$ . Now for this state of affairs to exist there must be three distinct things. If any two are the same we have congruence, not inequality. It is this inequality ( $NRV > PV$ ) which Popoff calls impossible, maintaining that  $PV \geq NRV$  (because  $NRV$  is only one form of  $PV$ , i.e.,  $NRV \equiv PV$ ). But this violates the inequality above and is wrong.  $PV$  does not stand for 'present value' but for 'present value in use' and there is no reason at all why value in use should equal value for sale—in fact the reverse applies.

As in the case of the schoolboy's 'proof' that  $1 = 2$ , there is always this danger that an abstract argument will proceed to an apparently undeniable conclusion only by passing through an untenable stage or by 'drifting' from the original definitions of the symbols. It is therefore advisable to apply *each stage* of a final proof to a concrete example.

I buy from my local antique shop one of two matching ornaments, at a barely acceptable price of £50, so that  $RC = PV$  (enjoyment value). By the time I reach home a smug satisfaction produces  $PV > RC$ . However, after some time I get tired of the thing and ask the dealer to buy it back. He offers £25. I am not that tired of it and so take it home:  $RC > PV > NRV$ . A friend calls, takes a fancy to the ornament, and offers me £45:  $RC > NRV > PV$ .

But let us continue. The night before the ornament is collected I break it. Not wishing to appear a clumsy oaf, or to disappoint my friend,

I hasten to the dealer who agrees (after my 'sob-story') to sell me the second ornament for £40.

Now we have:  $NRV > RC > PV$ .

Thus we establish that this state of affairs is *not* impossible.

What is my loss from the breakage?

Popoff would have that it is £45 ( $NRV$ ). It is, however, clear that in this instance I lost only the £40 it cost me to replace it for sale at £45. Where has the fallacy arisen?

As Gee and Peasnell state, nowhere is instantaneous replacement required; this idea is introduced by Popoff without any justification. Furthermore, the formula states that for greatest financial benefit the asset *should* be sold then *replaced and resold*, not that it *must* be, still less (and here is the nub of the matter) that it *can* be (and the action repeated *ad infinitum*) in the real world.

The assumption that  $NRV > RC > PV$  must apply before *and after* a sale is invalid. As the foregoing parable shows, after the first sale there might be no other buyer ( $NRV = 0$ ) or there may be no other supply ( $RC = \infty$ ).

It might be noted that even if a limited series of replacement were practicable then the value to the owner is still not  $NRV$  (as stated at the end of para. 1, p. 311) but the proceeds from *all* the sales. The unrealistic nature of this is admitted in Popoff's final sentence.

Finally, there is no need for Gee and Peasnell to consider delay in replacing the asset. If replacement is not possible before  $NRV$  falls below  $RC$  then *at balance sheet date* the situation is *not*  $NRV > RC > PV$  but is  $RC = \infty > NRV > PV$ .

# The Variabilities and Correlations of Stock Market Indices

Michael Theobald and John Whitman

## I Introduction

The variabilities and correlations of three stock market indices are analysed for the period from 1962 to 1976 in this paper. Stock market indices are of considerable importance in the field of financial economics. In the practical development of portfolio selection [1] index models have been developed [2, 3] to afford a reduction in the amount of computer space necessary to perform a portfolio analysis.<sup>1</sup>

A large number of empirical analyses in financial economics utilise a residuals approach,<sup>2</sup> i.e., instead of studying absolute returns on individual securities, the residual return after extraction of the market factor is used.<sup>3</sup> In general a stock market index is used as a proxy for the return on the market.<sup>4</sup>

The relevant dispersion measure for fully diversified (i.e., efficient) portfolios will be that of the market as a whole. The variability of the market, as proxied by the variability of the stock market indices, is consequently a parameter of considerable interest.

The three stock market indices analysed were *The Financial Times* Industrial Ordinary Share Index, *The Financial Times*—Actuaries '500' Index and *The Financial Times*—Actuaries 'All Share' Index. The levels of these indices are frequently quoted by the news media and financial community, and as such, represent proxies for the

'state of the market'. However the indices differ in their construction. Indices may in general differ in their construction in three important ways. They may differ as to the sample of stocks upon which they are based, in the weighting of stocks within the sample and in the method of averaging of the stock prices within the index (see, for example, Lorie and Hamilton [6]).

The FT Ordinary Index represents the geometric mean of a sample of thirty of the larger industrial and commercial companies. The FT-A '500' Share Index is a weighted arithmetic average of 500 industrial and oil stocks; the weights are proportional to the total market values of the shares outstanding at the base date (April 1962), but with subsequent modification to ensure continuity. The FT-A 'All Share' Index is constructed in a similar fashion to the '500' index, except that it includes, additionally, the financial group category in the index, the total number of stocks in the index being six hundred and fifty.

The monthly closing levels of each of these indices from April 1962 onwards were studied.<sup>5</sup> Data for the FT-A indices were obtained from the London Graduate Business School Share Price Data Bank, while the FT-Ordinary data was collected manually from *The Financial Times*.

In Section II the correlations between each of the indices are presented for the non-overlapping periods April 1962 to August 1969 and September 1969 to December 1976, and for the whole sample period. The sample period was partitioned into two sub-periods to investigate the stability of the relationships.

In Section III the coefficients of variation<sup>6</sup> for each of the three indices are given for a moving

<sup>1</sup>There will be  $(N^2 + N)/2$  parameters to be estimated in the variance/covariance matrix for  $N$  securities. Use of Sharpe's 'diagonal model' [2] reduces the number of parameter estimates to  $2N + 1$ .

<sup>2</sup>This technique was first reported in the now classic Fama, Fisher, Jensen and Roll [4] article. Numerous subsequent studies have used this technique.

<sup>3</sup>It is a fundamental property of OLS that the residuals from a regression represent the dependent variable after abstraction of the effect of the independent variable(s).

<sup>4</sup>Stock market indices do not include dividend variables. Fisher [5] has developed an investment relative index (i.e., an index including dividends) for the US market.

<sup>5</sup>The FT-A indices were first produced in April 1962; the FT-Ordinary index was first calculated in 1935.

<sup>6</sup>The coefficient of variation is defined as the ratio of the standard deviation to the mean. It is a measure of relative dispersion.

**Table A**  
**Correlation Matrix, April 1962—December 1976**

	<i>FT Ordinary</i>	<i>FT-A '500'</i>	<i>FT-A 'All'</i>
FT Ordinary	1.0000	0.8809	0.8507
FT-A '500'	0.8809	1.0000	0.9943
FT-A 'All'	0.8507	0.9943	1.0000

**Table B**  
**Correlation Matrix, April 1962—August 1969**

	<i>FT Ordinary</i>	<i>FT-A '500'</i>	<i>FT-A 'All'</i>
FT Ordinary	1.0000	0.9708	0.9613
FT-A '500'	0.9708	1.0000	0.9971
FT-A 'All'	0.9613	0.9971	1.0000

**Table C**  
**Correlation Matrix, September 1969—December 1976**

	<i>FT Ordinary</i>	<i>FT-A '500'</i>	<i>FT-A 'All'</i>
FT Ordinary	1.0000	0.9788	0.9711
FT-A '500'	0.9788	1.0000	0.9949
FT-A 'All'	0.9711	0.9949	1.0000

time-series of 12 month periods for the whole sample period.

There is a certain amount of evidence that suggests that stock prices follow Non-normal Stable Paretian Distributions [7]. A distinguishing feature of these distributions is that the variance does not exist. In order to calculate the correlation coefficient it is necessary to calculate variances. However, generalisation of Wise's [8] result indicates that the sample estimate of the correlation coefficient will be consistent, though inefficient.

Officer [9] found that the standard deviation of stock returns was 'well behaved' in the sense that his sample estimates did not vary erratically. This result provides a justification for the use of standard deviations<sup>7</sup> as a measure of the variability of the stock market indices. Further support is provided by the results of Blattberg and Gonedes [10] who found that for daily rates of return the Student model has a greater descriptive validity than that of the symmetric stable model.

## II Results—correlations of market indices

The correlations of the FT Ordinary, FT-A '500' and FT-A 'All Share' Indices were measured via the Pearson product moment correlation coefficient. In view of their differing constructions the

three indices would not be expected to be perfectly correlated, but, on the basis of studies in the USA [11], relatively high and positive correlations would be expected (a correlation coefficient of 1.0 indicates perfect and positive correlation, of -1.0 indicates perfect negative correlation and 0.0 indicates a lack of correlation).

Tables A, B and C set out the correlation coefficient matrices for the whole period and the two sub-periods.

The correlations between the indices were uniformly high and positive for the whole period and the two sub-periods. For instance, 98.9% of the variation in one FT-Actuaries Index is 'explained' by the variation in the other FT-Actuaries Index.<sup>8</sup> As might be expected the correlations between the FT-Ordinary Index and the FT-Actuaries Indices were somewhat lower—for the whole period, 77.6% of the variation in the FT-Ordinary Index is 'explained' by the FT-Actuaries '500' Index and 72.4% is explained by the FT-Actuaries 'All Share' Index. Higher correlations were found in the two sub-periods again.

The indication is, then, that over the period covered by this study, the three stock market indices were telling substantially the same story and that this phenomenon was particularly the case when the sample period was broken down into two subsets.

<sup>7</sup>And the coefficients of variation. The results in Section III are consistent with Officer's observations.

<sup>8</sup>The corresponding values for the two sub-periods were 99.4% for the first sub-period and 99.0% for the second.

### III Results—variability of indices

Tables D, E and F contain the standard deviation and coefficients of variation for the whole sample period and for the two sub-periods, respectively.

**Table D**  
Standard Deviation and Coefficients of Variation,  
April 1962—December 1976

	Standard Deviation	Coefficient of Variation
FT Ordinary	73.86	0.217
FT-A '500'	36.64	0.262
FT-A 'All'	37.32	0.279

**Table E**  
Standard Deviations and Coefficients of Variation,  
April 1962—August 1969

	Standard Deviation	Coefficient of Variation
FT Ordinary	61.49	0.171
FT-A '500'	26.26	0.217
FT-A 'All'	23.72	0.210

**Table F**  
Standard Deviations and Coefficients of Variation,  
September 1969—December 1976

	Standard Deviation	Coefficient of Variation
FT Ordinary	84.21	0.226
FT-A '500'	35.86	0.226
FT-A 'All'	36.87	0.238

For the whole sample period the FT-Ordinary share index was significantly less variable than either of the FT-Actuaries Indices (it was 17% less variable than the FT-A '500' and 22% less than the FT-A 'All Share' Index). This result is a little disquieting given that these indices should be proxying the 'market' return and condition. In the second of the two sub-periods, however, the variability of all the indices is broadly comparable, due to the very volatile state of the market as a whole in the period 1972 to 1974 (the coefficients of variability are larger in the second sub-period for all three indices).

The coefficients of variation for the 12 month moving series are shown in Fig. 1. The 12 months moving series was constructed by calculating the standard deviation for the first 12 months' data, then the first month's data were excluded and the 13th month's data included and the standard

deviation re-calculated.<sup>9</sup> This procedure was repeated for the whole sample period. The coefficient of variation for the moving series was then calculated.

Fig. 1 demonstrates the general similarity of the variability of each of the indices and, in particular, it indicates the extreme variability of the indices from 1973 to mid-1974. This range of variability was quite unprecedented in the previous period from 1962 and afterwards to the end of 1976. The extreme nature of these market-wide fluctuations indicates the potential problems that would be present in experiments involving share price responses which do not extract market-wide share price movements.

In the falling market in 1972/73 the FT-Ordinary Index was slightly more volatile than either of the FT-Actuaries Indices while in the rising market of 1974 the FT-Ordinary Index is slightly less volatile than either of the FT-Actuaries Indices. This is in accord with the mathematical property of geometric indices that they fall more swiftly than the corresponding arithmetic indices but rise more slowly than the corresponding arithmetic index.<sup>10</sup>

### IV Conclusions

It was found in the second part of this study that the correlations between all three indices were uniformly high and positive. The implication for empirical studies using the market model to abstract from market-wide stock price movements is that the choice of an appropriate surrogate market return is not a decision of crucial importance.<sup>11</sup> It is stressed that while the choice of an index may not be crucial for residuals analyses, the determination of the market factor for capital asset pricing tests is of crucial importance. (See Roll [14].)

In the third part it was found that the variability of the FT-Ordinary Index was somewhat less than that of the FT-Actuaries Indices, and that there was considerable volatility present in the period 1972 to 1974. This led to the conclusion that any studies conducted in this particular period<sup>12</sup> which did not specifically adjust for market wide movements would have a somewhat

<sup>9</sup>This technique was also used by Officer [12].

<sup>10</sup>The differing compositions of the indices would be another explanatory factor.

<sup>11</sup>Marsh [13] however provides empirical evidence that caution should be exercised when using the FT-Ordinary Index for beta estimation.

<sup>12</sup>And other periods as well.

low internal validity (i.e., attempts to observe the effects of particular information sources or phenomena upon stock prices would tend to be distorted by market wide movements).

The generally non-erratic behaviour of the sample estimates of the coefficients of variation indicated that the second-order moments of the distributions did exist.

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**Acknowledgement** We are grateful to Professor Bryan Carsberg for his comments on an earlier draft of this paper. We retain responsibility for any remaining errors or lack of clarity.

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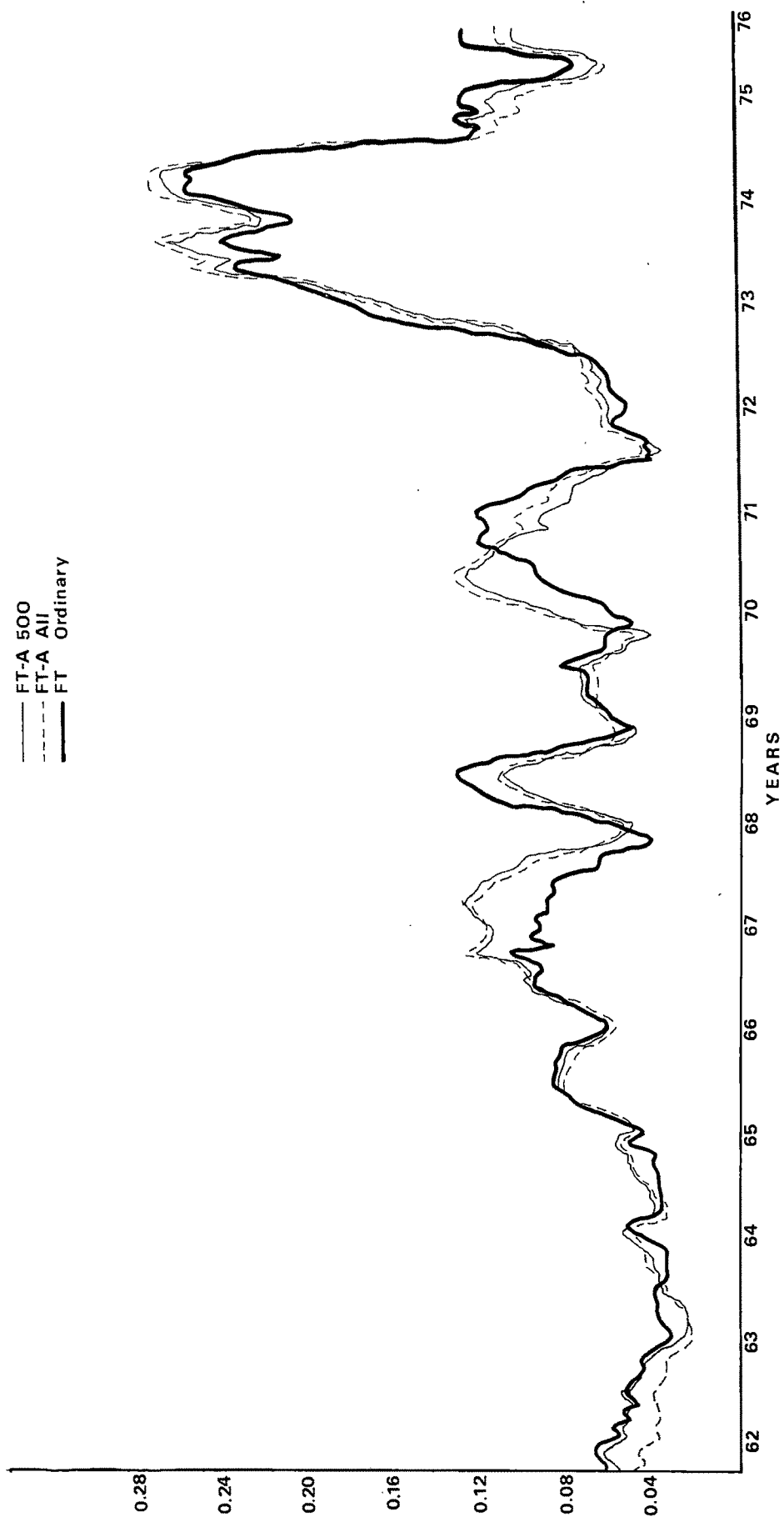


FIGURE 1  
Coefficients of Variability of 12 month moving series



## Book Reviews

### **Current Cost Accounting: Identifying the Issues.**

*Edited by G. W. Dean and M. C. Wells.* International Centre for Research in Accounting, Lancaster, and Department of Accounting, University of Sydney, 1977. 196 pp. £5.50.

The literature on 'inflation' or 'price-level' accounting has now reached such proportions that few of us can affirm that we are familiar with the lot. The Editors of this collection have usefully brought together in one cover, not perhaps a representative selection (that would be hard to define and select) but at least a telling collection of writings, giving a perspective of the current debate and a flavour of the range of contributions over the years from the early 1900s.

The main schools and sub-schools are represented: General Price Level, Replacement Cost, CCA, COCOA, not forgetting 'HC is the only factual and true method'. The quality and style ranges from superficial journalese to the first-class intellectual essay, from the largely theoretical to the exposition of convincingly real practical difficulties.

It is impossible in a short review to deal with all the contributions whose intellectual level deserves mention, so I shall confine myself to referring to some I found among the most interesting.

Zeff in his article on replacement cost (*Accounting Review*, October 1962) provides an excellent review of the price index (general and special) question, and more generally of the problem of improving financial accounting. He assumes the need to separate out the effects of general price inflation (or deflation); he discusses the 'realisation' problem that arises when replacement costs are brought in; and he suggests a rationale for replacement cost based on the assumption that under competition replacement cost will tend to approach discounted present value.

Burgert's article (*Abacus*, December 1972) is essentially a criticism of Limperg's crude replacement cost model. It sums up admirably the main problems posed by a Sandilands calculation of profit, and of a modified Sandilands incorporating a monetary adjustment based on specific price

changes. It is written at a level of economic understanding too often missing in this debate. Burgert notes incidentally that relatively few Netherlands firms have in fact used a Limperg model.

Lemke (*CA Magazine*, November 1976) examines the weakness of the Sandilands income measurement, based as it is on the maintenance of the 'value to the business' of a constantly changing mix of assets. So far as asset valuation is concerned, he points out that Sandilands' 'value to the business' does not cope adequately with technological change (though claiming to do so *via* the use of index numbers). Lemke also points out that, as others have suggested, some changes in replacement cost may have predictive value, and that in any case isolation of holding gains in reserves is not essential to give a better basis for prediction of a firm's future prospects. But, as he says, in the end predictive power must be tested empirically.

Swan in 'CCA and Taxation' (*Australian Financial Review*, April 6, 1975) attacks CCA on the obvious grounds that the system ignores real holding gains and losses, and argues by implication that a general price-level adjustment is essential for equity in a system of taxation based on attempts to measure changes in wealth or sub-sets of wealth. The article also suggests, somewhat oddly to my mind, that COCOA would provide a practical approach to a Hicksian system which would be both equitable and simple. The mind boggles at the problem of arguing annually with the tax inspector about the 'cash-equivalent' resale values of the assets of Esso or Broken Hill Proprietary.

Rosenfield's article on 'Current Replacement Value Accounting' (*Journal of Accountancy*, September 1975) provides a clear and well-reasoned analysis of the weakness of the case for CRVA (most of which applies equally to CCA), dealing with two widespread fallacies, namely that economic welfare requires that every business should continue to exist at not less than its present level, and that increased (or decreased) liquidity can be identified with increased (or decreased) 'well-offness'.

The collection taken as a whole is hardly suit-

able for the average undergraduate though good students could benefit from selected reading in it. It could certainly be used to advantage by the bright graduate on a masters' course, or by any person reasonably informed on accounting matters who wished to get an idea of the range of arguments on the topic and how good (and bad) they can be.

The Editors' introduction and the articles give many useful references.

London School of Economics      Harold C. Edey

**The Management Style of the Chief Accountant.**  
*Eugene F. McKenna.* Saxon House, 1978.  
xiii + 307 pp. £9.50.

The increased attention being focussed upon the role of management accountants in corporate life is a most welcome development in the accounting literature. To date that role as it has developed and as it presently exists, has been poorly defined and only partially understood. Further empirical study must certainly be an essential prerequisite to any normative prescriptions in this area. In view of these observations, Eugene McKenna's study is a most welcome advent and likely to be of interest to researchers and postgraduate students.

Ostensibly, the study is designed to enhance our understanding of chief accountants' leadership styles and to make a contribution to the development of situational leadership theory. Its contents are briefly outlined here. Accountants are examined in terms of a distinct profession and in terms of the type of work done by chief accountants. Leadership literature is considered with respect to trait, style, effectiveness and relevant situational factors. Decision processes and corporate structure are similarly considered. The empirical research design, hypotheses and methods of statistical analysis are then explained at some length. Results of questionnaires and interviews are disclosed and a separate discussion of results in relation to each hypothesis is then provided. The conclusion includes a summary of results and practical implications of the whole study. Appendices include the questionnaire instrument used, various tabulated results and the interview schedule used. A comprehensive bibliography is also provided.

Unfortunately the book fails to realise its full potential. In the first instance it suffers from

several fundamental weaknesses. The introduction and statement of objectives are far from clear, the writing style is ponderous, section headings are too few and unrepresentative of their content, the author's own arguments rarely emerge from the mass of literature and empirical evidence, early chapters fail to generate a coherent theme or to provide a traceable flow of argument and many terms and parameters used by the author are not satisfactorily defined. Even for a reviewer experienced in the areas of organisation theory, administrative theory and behavioural accounting, this text makes for very heavy going and much rereading of sections in an effort to catch the author's drift.

On no account, however, should this book be lightly cast aside. Its strengths lie in its comprehensive summary and analysis of a whole range of literature in the management and organisational fields, its exceptionally well documented research design and its well integrated discussion of hypotheses, statistical results and supplementary evidence. Reasons for and explanations of research techniques used, statistical tests and cross checks used are provided in one of the clearest and most comprehensive presentations that the reviewer has yet encountered. For reasons outlined in the book, the author had to rely to a considerable degree upon parametric statistics in his analysis of results but was able to confirm his test results with non-parametric tests. While alternative non-parametric tests might have been usefully added or substituted in some instances, the general statistical and research methodology has been well conceived and is soundly based.

Results are outlined in terms of relationships between chief accountants' decision style (the dependent variable) and a range of independent variables such as types of decisions, experience, specialisation, subordinate skills, status, span of control, size of organisation etc. Statistical tests of strength of correlation between variables are commented upon and usefully supplemented by selective quotations from interviews. Relationships were tested using the Normal Statistic, Coefficient of Multiple Correlation, Chi Square, Partial Correlation, Pearson's Product Moment Correlation Coefficient and Kendall's Tau Rank Order Correlation Coefficient.

Examples from the summary of findings are as follows. The Chief Accountant places equal emphasis upon restrictive and permissive decision styles, uses both directive and delegative decision styles for programmed and task decisions, gravitates towards a more directive style with the pass-

ing of time, uses a directive style when confronted by a high level of specialism or a wide span of control and so on. The reader might be forgiven for thinking that many of these findings are not particularly remarkable and might have been largely anticipated prior to this study by at least behavioural accounting researchers and probably many practising accountants.

Having attempted to outline the objectives, contents and general strengths and weaknesses of this book, some specific criticisms must be made. The problem of few and unrepresentative headings is aggravated by the cheap form of printing used by Saxon House which only allows about half the number of words per page achievable through the more normal type setting format. The whole physical appearance of the book is second-rate and gives the distinct impression of a type-written MSS stuck inside a dowdy cover. This matter is of course beyond the author's control and is probably simply a reflection of hard times in Britannia.

The title of this study is 'The Management Style of the Chief Accountant' and yet the introduction discusses only leadership style and the results are discussed only in terms of decision making style. Does the author equate all these? If so, how? The author's view of the relationships between management style, leadership, decision making processes, decision making behaviour and situational factors is not made clear and the possibility of reciprocal relationships remains unexplored. How these concepts are integrated with each other or differentiated from each other is not discussed. As an exploration of management style, the study appears to adopt a somewhat restricted outlook in concentrating mainly upon five decision styles from one decision style continuum.

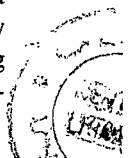
Another limitation of this study is that its questionnaires and interviews were administered only to Chief Accountants. Such a study might reasonably be expected to include the observations of a whole range of corporate personnel in order to obtain a more balanced and more reliable view of the management styles of Chief Accountants. A Chief Accountant's perception of his decision styles might be quite different from the perceptions of his styles by superiors, peers and subordinates. The results of this study must therefore be treated with a deal of caution.

Ill-defined terminology and the lack of coherent theme and argument cause considerable problems in literature review chapters 2 and 3. "Decision Making Behaviour and Situational Factors" (chapter 2) moves from leadership to participation

to a leadership continuum. The reviewer found this a somewhat restrictive and unusual treatment which omitted a considerable area of behavioural and quantitative decision theory literature. Again it is unclear as to what this chapter concludes or how it leads to chapter 3. Chapter 3, 'Impact of Decision Processes and Structural Determinants', also presents some difficulties. It lacks clarity and fails to produce a coherent argument. Within this chapter, the section 'Structural Determinants' does not relate clearly to the book's theme and is far too broad and superficial in its sweep of the literature. The reader moves in rapid succession from Weber's bureaucracy, to the Merton/Gouldner/Selznick critiques, to Urwick's summary of the classical-scientific management school to Woodward's concern for corporate environment and technology to Burns and Stalker's organic-mechanistic organisation to Emery and Trist and Lawrence and Lorsch's environmental concerns to communication and finally to organisational design. All of that occurs in the space of  $7\frac{1}{2}$  pages!

Chapter 3 is concerned with structure, attitudes and behaviour (according to its headings) and yet the author encompasses a whole range of concepts and relationships, few of which are adequately defined. Organization structure is examined for its effect upon alienation, personality and group effectiveness. Hierarchical level effects upon behaviour, self-perception, and job satisfaction are considered. Organization size is studied for its effect upon morale, satisfaction, interpersonal relations and communication. Which of these does the author see as components of attitude or behaviour? Which concepts is he attempting to specify as dependent variables or independent variables? Which concepts does he see as interrelated? Some recognition of the complexity of such relationships would be in order here. Complexities such as the observation that attitude affects behaviour and that behaviour also affects attitude are to be ignored at the researcher's peril. Furthermore such a study must take care to specify whose attitudes to whom or what should be considered (e.g., accountant to job, subordinate to accountant, production director to accountant).

By the time the study sets up and evaluates its hypotheses, management style has come to be assessed simply in terms of five decision styles which does seem to be a rather restricted definition of management style. Even then, quite a number of hypotheses can only be cautiously accepted and the reader is left with the feeling that further determinants of the dependent vari-



able (decision style) remain to be specified and tested. A more satisfactory view of management style, however, might be achieved through studies of Chief Accountants' styles of communication, approach to superiors, behaviour in peer group decision settings and reactions to change, bearing in mind that other organisation members' perceptions will provide important guides.

Symptomatic of the book's problem with theme and argument is the return to the subject of participation in the 'Conclusion' chapter. By what process the book arrives at this point is unclear. The chapter also contains 'Practical Implications' in which the author states that 'The evidence derived from the research certainly poses a serious challenge to the view that a particular leadership style is suitable irrespective of the prevailing organisational circumstances'. The reviewer would submit that such a view has not been commonly held by academics or managers for a considerable number of years, so that there is no object for this particular challenge. Finally the author argues against placing an undue emphasis upon interpersonal behaviour in seeking effective leadership strategies. From where in the study the evidence for this view is derived, is not at all clear. This critical issue however is raised and left in a matter of five or six sentences, and only one page from the end of the text.

All in all, this is a fascinating yet somewhat frustrating study which however must be accorded due respect as a path-breaking attempt in the behavioural accounting literature. While it is probably too bewildering for the undergraduate or practitioner, it is a tantalising resource for researchers bent upon the development of this area of study. In spite of its difficulties it is a welcome resource.

Monash University

L. D. Parker

**Capital Investment and Financial Decisions** *H. Levy and M. Sarnat*. Prentice Hall International, 1977. xiv + 354 pp. £8.95.

Any reviewer faced with yet another introductory book in this area must ask what differentiates the book being reviewed from its large number of competitors. The authors of this book surprisingly do not in their preface help the reviewer in this task. One differentiating characteristic of this American book which may make it of use in Britain is that it contains little institutional matter and therefore tends to fit better into teach-

ing practice in the United Kingdom where the general emphasis is on the analytical part of the subject. The other unique characteristic of this book is that although it covers the usual material found in modern finance books it does so in a relatively simple way. Much of the more complex material is put into the appendices. Even here the material is less mathematical, and, perhaps, less critical than now seems to be the custom. The presentation at this level is superb, and, indeed, may give the impression that the subject is easy.

The first chapter of the book considers the goal of the firm. This explanation of the possible goals of the firm is at a fairly low level. Some will not find the authors' reasons for choosing wealth maximisation convincing. The reason for this choice is not put in a very analytical framework. An example of the problems caused by relegating most symbol manipulation to appendices is that the proof that wealth maximisation is equivalent to the maximisation of the market value of a firm's common stock is in Appendix 14 A. An example of the problem generated by attempting to present difficult abstract concepts in a simple to follow way is that the main text does not say that this equivalence is not general. Chapter 2 is concerned with describing the capital expenditure process. In common with most of its competitors, the book says little about project generation which many would think more important than the more technical aspects of capital budgeting theory. Chapters 3 and 4 consider the economic evaluation of investment proposals and compare and contrast the NPV and IRR decision rules. The former subject is dealt with straightforwardly but without any real analytical treatment—Hirshleifer's work seems not to figure in the main text. The latter chapter gives the usual presentation of the comparison between the two decision methods. Levy and Sarnat seem still to place considerable emphasis on reinvestment rates as an explanation of the different rankings that may be given by the two methods.

Chapter 5 explains the use of incremental cash flows in investment decision making. Items which may be regarded as important in the United Kingdom are treated superficially—the discussion of opportunity cost takes less than a page. The next chapter gives a good elementary treatment of taxation and capital budgeting. Chapters 7 and 8 complete the first section of the book. The first deals with applications of capital budgeting and the second considers what are now regarded as traditional methods (non-discounting methods) of project appraisal.

The second part of the book consists of five chapters dealing with the treatment of risk and uncertainty. This section emphasizes portfolio theory and the capital asset pricing model and provides a very well written introduction to these and other areas of risk and uncertainty. Again the treatment is relatively less deep than that in competitive books; that one is dealing with subjective probabilities is not well explained. The reasons for the use of utility functions are explained mainly via examples and illustrations. The treatment of the CAPM hardly deals with the difficulties of extending the model to what some would regard as more realistic situations. The subject of efficient capital markets does not figure in the book.

The final section of the book deals with financing decisions in a fairly traditional way. Chapter 14 considers the fundamentals of leverage and provides a good treatment of the M & M thesis and uses a very good simplified approach based on the work of Stiglitz. The following chapter deals with the complications introduced into the analysis of financial structure by the possibility of bankruptcy. There then follow two chapters of the traditional kind dealing with the cost of capital. This is the case even though the students have already been exposed to portfolio analysis and the CAPM and should therefore have grave doubts as to whether the traditional view of the cost of capital is of utility. This criticism also applies to the last chapter of the book which deals with inflation and financial management. Again one might be disappointed that modern developments in analytical theory are not utilised in dealing with this problem. Chapters 18 and 19 deal with capital rationing and dividend policy in the usual way. Again, the treatment is less demanding than in some other books. For example, little is said about the problem of defining the discount rate in a capital rationing situation. The various suggestions for overcoming this problem are not reviewed.

In sum this is a very clear introduction to business finance but cannot serve as a basic textbook for the better second and third year undergraduate financial management courses in the United Kingdom.

University of Reading

Michael Bromwich

**Capital Budgeting Techniques.** *F. M. Wilkes.* John Wiley & Son, 1977. ix + 424 pp. £13.

It is always a pleasure to read a book by an

author who has complete command of his subject. Pleasure is the greater when the subject is one as difficult as the use of mathematical techniques in capital budgeting and the author is British rather than American. In this nicely set out book Dr. Wilkes reviews several mathematical techniques which have been found to be of considerable power in solving problems in capital budgeting. If the reader wishes to obtain a good introduction to a variety of mathematical programming this book meets his needs.

The author claims that the prerequisites required to understand the book are minimal. This is reasonable for all but those who either have symbol-blindness or lack the ability to concentrate, though later parts of the book do deal with some fairly sophisticated models which may tax the patience of many a mathematical novice. Some may doubt whether this approach of assuming no mathematical knowledge is the correct one for it does involve the author in giving some terse descriptions of mathematical tools such as elementary calculus and matrix algebra. One cannot entirely escape feeling that a better use of the author's undoubted talents would be to introduce more numerate students to fairly sophisticated capital budgeting theory.

The book describes the mathematical models in some detail and explains solution techniques. It deals with those problems, such as degeneracy and integer requirements, which are often wrongly regarded as of technical interest only. This comprehensive treatment of the models reviewed is achieved at the substantial cost of abstracting almost entirely from the financing side of capital budgeting except under conditions of capital rationing. The grounds for doing this are not clear. Certainly there are sufficient sophisticated mathematical models in the financing area which require someone to lead the mathematical layman to a clearer understanding of the models.

The book concentrates mainly on project accept/reject decisions and the solutions to the problems of ranking projects. The first two chapters provide a terse treatment of the fundamentals of capital budgeting. In less than 60 pages the basic theory and many of its refinements are explained. This brief treatment is a pity for it allows neither proper references to the underlying analytical framework nor a good treatment of items like project truncation and the effects of incorrect estimates of variables which are dealt with infrequently elsewhere.

The author's major contribution is a very fine treatment of the mathematics of programming

models. Chapter 3 introduces linear programming. The next chapter gives a good description of dual programmes and their uses and emphasises their economic meaning. This provides a very good introduction into the problems of 'hard' capital rationing. There is no treatment of other types of capital rationing. The presentation is excellent for all those who wish to understand the computational mechanics of the problems and leads to a much clearer understanding of sensitivity analysis, the importance and computational requirements of which are stressed in many chapters of the book.

Chapter 6 presents the essence of several programming based corporate planning models. The collection of these together in one place is a service to the academic and business communities. But it is a pity that the author neither deals with goal programming which many think holds considerable promise for the realistic modeling of this area nor compares the results of using analytical models with those promised by simulation approaches. Chapters 7 and 10 complete the list of programming models dealt with. The former gives a good presentation of integer linear programming. The latter gives a brief introduction to the more advanced topics of non-linear and stochastic programming. It is not clear why the author did not complete his product range and deal with dynamic programming. This has clear application to the area of concern and there is a crying need for a simple explanation of this technique.

Of the remaining chapters, two deal with important areas that are usually overlooked in capital budgeting books. Chapter 8 considers the linking of DCF techniques and critical path methods and chapter 9 deals with distribution problems and indicates their relevance to capital investment decisions thus providing an important lesson to many writers in this field. The last chapter of the book, chapter 11, introduces portfolio theory, concentrating mainly on the programming aspects. This treatment of portfolio theory is very brief for those who are coming to it fresh and one wonders whether this will militate against the chapter achieving its objective.

The book does not lend itself to use as a course book in business finance or financial management because of its restricted coverage. It is an important book for those who teach in the area covered and an indispensable reference book. The emphasis on mathematical and computational techniques does mean that those who wish to stress analytical reasoning supporting the use of

the various models will have to supplement the book with references to other literature. The emphasis on solution techniques also raises the pedagogic point of how much manual solution technique we wish to give students who, one assumes, in practice will have access to a computer and relevant software packages.

University of Reading

Michael Bromwich

**Accounting Information Systems and Business Organisations.** *Barry E. Cushing.* Addison-Wesley, 1978. xi + 550 pp. £12.75.

The author states in his preface that 'This book attempts to integrate coverage of computer-based systems with coverage of accounting implications. The intent is to provide a foundation for the development of today's accounting students into tomorrow's users, auditors and managers of information systems.' The book is divided into four parts:

- Part 1—Concept of Accounting Information Systems
- Part 2—Technology of Information Systems
- Part 3—Management of Information Systems
- Part 4—Accounting Information System Applications.

The book adequately covers (in parts 2 and 3) the technology of information systems and the management of them. The author deals first with basic computer hardware and software, and then goes on to describe database and real-time systems and the differences between batch processing and real-time systems. In later chapters he describes the setting up of a computer-based systems function, and the evaluation, selection and implementation of computer-based information systems, including the use of PERT in implementing a system. A chapter is devoted to internal control in computer-based systems.

Parts 1 and 4 ('Concept of Accounting Information Systems' and 'Accounting Information System Applications') cover their subjects less adequately. The first chapter of Part 1 deals with the various information requirements of customers, suppliers, stockholders, employees, lenders and government, and also with the central role of the accountant in helping to meet these various information requirements. The author then deals with the concepts of organisation, budgeting (with a very simple, almost superficial dis-

tion between fixed and variable expenditure), accounts coding, and file processing. He deals finally with basic computer concepts, and with equipment that is currently available, including mini computers and other business machines.

Basic systems such as payroll preparation, stock control, sales analyses, production planning, accounts receivable and accounts payable records, etc., are described in considerable detail in latter parts of the book. The author does not, however, deal with the subject of management information, and he makes no suggestions about the type of information that senior management and directors should ask for and expect. No examples are given in the book of a management style profit and loss account and balance sheet, and, in fact, no examples are given of a traditional profit and loss account and balance sheet. Also, cash flow forecasting is not dealt with and no examples are given of source and application of funds statements. The subjects of corporate planning and long-term profits forecasting are not developed in detail. Future developments in information systems are mentioned (including inflation accounting and current value accounting) but these are not discussed in detail.

Part 4 of the book deals with applications of accounting information systems for marketing, purchasing, production, personnel and financial management, but the information requirements of the other member of the team, the chief executive, are ignored. Also the systems applications that are dealt with are the routine ones of sales order processing, raw material inventory processing, production planning, payroll processing, accounts receivable and accounts payable, and cost accounting. Cost accounting, which is a subject on which many accountants feel that they can be most useful to management, is dealt with in only a superficial way.

To summarise: this is a disappointing book. It contains much useful information on the mechanics of building an information system, but it seldom rises above the 'building bricks' level. It is unlikely to inspire today's accounting students to develop into tomorrow's users, auditors, or managers of information systems. However, it contains some useful basics, and the case studies and questions posed in the book should help to ensure that the basics of accounting systems are understood and remembered by those students who read it.

### **The Growth and Impact of Institutional Investors.**

*Richard J. Briston and Richard Dobbins.* The Institute of Chartered Accountants in England and Wales, 1978. 216 pp. £9.50

This is a highly topical book in view of the fact that the Wilson Committee is currently investigating the workings of the City and, in particular, the role of the financial institutions. The authors have done a valuable job in bringing together a mass of information about the growth of institutional investors and their impact on the UK stock market. They are, however, the first to admit that the book does not contain a great deal that is new. They list as many as nine earlier articles on the same general subject that they have published either separately or together or in collaboration with other authors. One of these, a 35 page booklet by Richard Dobbins and T. W. McRae entitled *Institutional Shareholders and Corporate Management*, was reviewed in the Spring 1977 issue of *Accounting and Business Research*.

The heart of the book lies in the 85 pages of tables. These are so detailed and comprehensive that the book is bound to become the standard work of reference on the subject. It is, therefore, a pity that, due to the inevitable delays of publishing a book of this length, many of the tables already look out of date. Most of them end in either 1975 or 1976. Perhaps the Institute's Research Committee should be thinking of asking the authors to produce a second edition. If so, the opportunity might be taken of reducing the length of some of the text. This contains a wealth of interesting material but occasionally one finds that the authors are describing the contents of the tables almost line by line. A cross-referencing system would also help, so that the reader could find the text that related to each table.

Institutional investors are defined as insurance companies, pension funds, investment trusts, and unit trusts. These four classes of investor are shown as holding 43% of UK listed equities at the end of 1975. This total is, however, increasing at some 2% a year, largely at the expense of private investors, and is almost certainly over 50% by now. The authors argue that this trend is likely to lead to less efficient and more volatile stock markets and, eventually, to the need for institutional investors to become more involved in the management of the companies in which they invest.

They point out, however, that different types of institutional investor behave in very different

ways. For instance, insurance companies and public pension funds are relatively inactive investors, judged by the proportion of their equity portfolios sold each year, but investment trusts and other types of pension funds are as active as the market generally and unit trusts have well above-average rates of activity.

The authors say that higher levels of portfolio activity are not associated with better portfolio performance. In fact, they are generally unflattering to investment managers. They point out that institutional investment performance tends towards the average (how could it do otherwise if institutional turnover represents over half the total and much of the rest is also based on professional advice?) and suggest that professional investment managers offer diversification, not performance.

One of the most interesting aspects of the book is the comparison with the situation in the United States. US institutional shareholdings account for only around 26% of the total and, contrary to the position in the UK, this percentage is growing comparatively slowly. On the other hand, US financial institutions appear to concentrate their shareholdings in the top 30–50 companies to a greater extent than happens in the UK. The authors regret the fact that much less information is published about stock exchange transactions in the UK than in the USA, a situation which they believe will not alter until the British Government introduces a Securities Exchange Commission to regulate the securities markets in this country. They also criticise the relative lack of research into the securities markets which has been undertaken in the UK.

A notable omission is any discussion of the role of institutional investors in other markets such as Government securities and property. Of the estimated £8 billion cash flow of pension funds

and life insurance companies in 1978, only £1.2 billion is believed to have been invested in equities, compared with £4.5 billion in Government securities, £1.0 billion in property, and £0.6 billion in loans and mortgages and other fixed interest securities. The balance represented a temporary increase in cash deposits.

The authors emphasise that, despite the resurgence of rights issues since 1974, much of the equity purchased by financial institutions comes from private investors and does not represent a flow of new money into industry. They are not convinced that British industry is short of finance but say that, if this is found to be so, 'mechanisms must be devised which channel funds from banks, insurance companies, pension funds, and perhaps even building societies into new industrial investment'. It is difficult to see what form these 'mechanisms' might take. Would, for instance, pension funds be required to invest a proportion of their members' money into British industry at yields below the market rate? (It must be assumed that the yields would be below the market rate for that particular type of investment, otherwise the investment would be taking place already.) If so, which type of investment currently being undertaken would be reduced to make up the difference?

The book ends with the comment that if the Government wishes to exert greater control over industry it will be possible to do this by 'nationalising insurance companies and assuming control of all pension funds'. No doubt the book is being studied closely by the Wilson Committee. The City will be hoping that this particular suggestion will not form part of the Committee's ultimate recommendations.

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## Contributors to Accounting and Business Research Volume 9 No 33 Winter 1978

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